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CONTENTS.

Original Communications.

PAGE

Clinical Therapeutics—Lectures on the Treatment of Hepatic Affections. By Professor Dujardin-Beaumetz..... 649
The Surgery of the Spine. By J. William White, M.D..... 656
Preliminary Note on the Pharmacology of Salicylamide. By W. B. Nesbitt, M.D..... 686
The Limitations of Bacteriological Therapeutics, with Special Reference to Tuberculosis of the Lungs. By E. P. Hurd, M.D..... 690

Leading Articles.

The Treatment of Granular Conjunctiva..... 695
The Theories of Immunity..... 697
The Physician as Surgeon..... 698
Prescription-Writing..... 699
A New Volatile Antiseptic..... 699

Reports on Therapeutic Progress.

The Action of Chloral Hydrate on the Kidneys..... 694
Benzoyl and Cinnamyl Eugenol..... 699
New Treatment of Diphtheria..... 700
The Hypnotic Action of the Antipyretics 701
The Use of Morphine for the Relief of Certain Uræmic Symptoms..... 701

PAGE

Treatment of Broncho-Pneumonia in Children by Subcutaneous Injections of the Hydrochlorate of Quinine..... 703
A New Treatment for Coryza..... 703
Fatty Degeneration of the Heart in Diphtheria..... 703
Subcutaneous Injections of Iron..... 704
Phenocoll Hydrochlorate..... 704
Gonorrhœal Salpingitis..... 704
Influence of Antipyrin on the Secretion of Milk..... 705
A New Treatment of Granular Conjunctivitis..... 705
The Influence of Enemata and Certain Remedies on the Composition and Quantity of the Bile..... 705
A New Mode of Administering the Bromides..... 705
The Active Principle of Wood Anemone..... 706
New Remedies in Whooping-Cough..... 706
Pancreatic Digestion of Fats..... 706
Injections of Nitroglycerin for Poisoning by Illuminating Gas..... 707
The Application of Hot-Air-Tar Baths in the Treatment of Rheumatism..... 707
The Action of Hydrastine upon the Vascular System and the Uterus..... 708
Effects of Medicinal Agents upon the Composition and Reactions of the Urine..... 708
The Treatment of Diabetic Coma..... 709

PAGE

Reviews..... 710

Correspondence.

London Letter..... 713

Notes and Queries.

Bryonia Alba as a Remedy..... 694
Influence of Alkalies on the Bile..... 716
Cause of the First Sound of the Heart... 716
Depilatory Powders..... 717
Borax and Chloral..... 717
Creosote given by the Rectum..... 717
For Rheumatism and Lumbago..... 717
Salicylbromanilide a Fraud..... 717
A New Anæsthetic..... 718
For Warts on the Skin..... 718
Digitalin and Iodide of Potassium..... 718
Rossbach's Expectorant Mixture..... 719
Treatment of Affections of the Ear with Beta-Naphthol..... 719
Bananas in Chronic Bronchitis..... 719
The Physiological Action of Rubidium-Ammonium Bromide..... 719
The Action of Strychnine on the Cerebrum..... 720
A New Solvent of Camphor..... 720
Treatment of Urticaria..... 720
The Local Application of Carbolic Acid Solution in the Treatment of Articular Rheumatism..... 720

Original Communications.

*CLINICAL THERAPEUTICS—LECTURES ON THE TREATMENT OF HEPATIC AFFECTIONS.**

By Professor Dujardin-Beaumetz,† Paris, France.

LECTURE II.—THE LIVER AS AN ORGAN OF ANTISEPSIS (CLINICAL AND THERAPEUTIC CONSIDERATIONS).

GENTLEMEN:—In my last lecture I showed you on what experimental proofs we may base our belief in the antiseptic functions of the liver, and the really important *rôle* which this organ plays as a destroyer of poisons. I intend to-day, in accordance with the order which I have marked out for myself, to study the clinical and therapeutic consequences which flow from these physiological properties; and, first of all, a question comes up for consideration whether we have any reliable clinical sign which may enable us to determine whether these antiseptic functions of the liver are disturbed?

When jaundice appears, we know well that the biliary functions of the liver are modified; the presence of sugar in the urine indicates perturbations in the glycosuric function. There are, moreover, general and local physical signs, which make known modifications in the hepatic circulation. We have not, however, any precise signs by which we may be able to affirm a normal or abnormal

* From advance sheets, revised by the author (translated by E. P. Hurd, M.D.). Lectures delivered in Cochin Hospital, Paris, France.

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state of the antiseptic functions of the liver.

Some have attacked the problem directly, and have thought themselves able to test the integrity of function of the hepatic cell by the presence of urobilin in the urine. Others have gone about it indirectly, and, relying on the fact that there is a correlation between the functions of the hepatic cell and the glycogenic function, they have sought a means of diagnosis in the production of experimental glycosuria. Still others have approached this problem of antisepsis of the liver by the new ways traced by Bouchard and his school, and have said that, in order to judge of the activity of the antiseptic functions, it suffices to examine the toxicity of the urine. We shall successively pass in review each of these methods.

The urobilin method originated in the researches of Hayem and his pupil Paul Tissier.* These writers have maintained that urobilin when appearing in the urine has always an hepatic origin, and that this substance results from a morbid functionating of the hepatic cells.

The testing of urobilin in the urine is extremely easy, and is very rapidly attained by the spectroscopic examination of the urine. The spectroscope employed, and which I here show you (Fig. 1), is for direct vision, and of moderate cost (the cost is but twenty-five francs), and it suffices to examine the urine with this spectroscope to see appear in the spectrum the characteristic band of urobilin; it is a band which appears between the rays *b* and *f* of Frauenhofer. In order well to observe this band, the urine should fulfil the following conditions: It should be freshly voided, as the urobilin undergoes modifications under the influence of oxygen; it should be limpid, and this is attained by filtration; lastly, it should be acid, and to obtain this acidity it suffices to add a few drops of acetic acid to the urine to be examined. You will simply apply the spectroscope to the test-tube containing the urine, and you will direct the apparatus to the side of the solar light, taking care to have the help of the bright clouds.

If you desire to measure the quantity of urobilin by the extent of the absorption-band, you must provide yourselves with tubes, all of which have the same dimensions. As for myself, I make use of the little glass-stoppered vial which I here show you (Fig. 2), and which always gives the same thickness to the stratum of liquid under examination.

FIG. 1.

Spectroscope for direct view.

When biliverdin exists at the same time as urobilin in the urine, the absorption-band

FIG. 2.

Vial for spectroscopic examination.

is much larger, and we see neither the violet nor the blue of the spectrum.

All the reactions, as you see, are very simple, and it suffices to have once seen the absorption-band of urobilin to recognize hence-

* Tissier, *Thèse de Paris*, 1889.

forth its presence in the urine. The only difficulty results from the simultaneous presence of urobilin and bilirubin. You can, however, by a very simple process, based on the very great diffusibility of urobilin, separate urobilin from bilirubin. For this it will be sufficient to let fall, drop by drop, and with precaution, some distilled water in the urine to be examined (the two liquids should not be commingled); the urobilin will pass into the water, while the bilirubin will remain with the urine, and on examining with the spectroscope both of these liquids, you will be able to detect in the water the presence of the urobilin.

These preliminaries being settled, we have now to ask if the presence of urobilin is indeed characteristic of deranged function of the hepatic cell. It is worthy of note in this connection that even Professor Hayem, who defends the hepatic origin of urobilin, admits the presence of this latter in patients affected with certain hemorrhages, and in particular cerebral hemorrhages. On the other hand, in a series of researches which I have undertaken in this hospital, but have not yet finished, I have failed to find urobilin in certain well-marked cases of hepatic affections; but I am disposed to believe it to be true that, with the exception of cases of cerebral hemorrhage, when urobilin appears in the urine, there is bad functionating of the hepatic cells; while the converse is not quite exact, for there may be affections of the liver without urobilin. At any rate, it is a very simple process to which you can always have recourse when you suspect any hepatic affection.

I come now to the second test process,—that of the induction of experimental glycosuria. I have, in fact, told you that, according to Roger, there is always a correlation between the glycogenic activity of the liver and its antiseptic functions. Therefore it suffices to know whether the liver accomplishes well its glycogenic functions in order to know if it is in condition to destroy actively the poisons of the organism. Unhappily this second clinical problem is very difficult to solve.

We know by experiments of Colrat, of Couturier, of Lépine, and of Robineaud that when the liver is cirrhotic, and glucose is introduced into the economy, that substance appears in the urine. Unfortunately, this experiment, which I have often reproduced, is far from giving results as positive as might be desired, and in many cirrhotic patients I have not obtained glycosuria. My results are absolutely confirmatory of those of Roger, who in fourteen trials of administering glucose to patients suffering various hepatic affections has eight times obtained positive and six times negative results.

I will even endeavor, when I come to speak of the glycogenic liver, to show you that one of the conditions of diabetes is integrity of the liver. We cannot, then, count on this procedure to help us to decide as to the good functionating of the hepatic gland as a destroyer of poisons.

Moreover, this question of alimentary glycosuria is very complex. Do not forget that, according to Bouchard, the liver pours into the blood every day eighteen hundred and fifty grammes of sugar, which are burned by the economy, and that this combustion results from multiple circumstances which, according to the season, and also to the periods of working or repose, cause the quantity of sugar burned in the organism to vary considerably. Moreover, I shall come back to all these points when I speak of the glycogenic liver.

The third test-process, as I have said, is based on the determination of the toxic coefficient of the urine. You all know the important contributions of Bouchard and his pupils to the study of the toxicity of the urine, and the consequences which he has derived therefrom from a clinical and therapeutic point of view. I will, nevertheless, briefly sum them up for you, for there are points important to be remembered if one would appreciate at its just value this new experimental method.

It consists in injecting by the vein of the lobule of the ear of a hare a given quantity of urine, so as to cause the death of the animal. This is what Bouchard has described under the name of *urotoxy*, when he would refer this quantity of urine necessary to kill the animal to one kilogramme of the living being. Bouchard, moreover, calls *urotoxic coefficient* the quantity of urotoxy which one kilogramme of man can fabricate in twenty-four hours; and in order well to impress this subject on your minds, I will proceed in your presence to practise one of these injections.

Here is the urine of a man weighing sixty kilogrammes; this is the urine of twenty-four hours, and measures twelve hundred cubic centimetres. On injecting fifty cubic centimetres of this urine per kilogramme of the weight of the body in the vein of the lobule of the hare's ear, the animal succumbs. Hence,

twelve hundred cubic centimetres will kill twenty-four kilogrammes of hare. Our man weighs sixty kilogrammes, and secretes urine enough to kill in twenty-four hours twenty-four kilogrammes of hare. We have only then to determine by a simple rule of three how much one kilogramme of man fabricates in twenty-four hours available for killing one kilogramme of hare. You will obtain as an answer, four hundred grammes.

Hence the quantity of urinary poison which one kilogramme of man can furnish in twenty-four hours to kill one kilogramme of living animal is forty cubic centimetres. This is almost the normal figure, which is about forty-six cubic centimetres. But in the pathological state it oscillates between two and ten cubic centimetres. As you see, we may define thus the coefficient of toxicity; it is the quantity of toxic matter which the unit of weight produces in a unit of time.

In the physiological state this urotoxic coefficient presents certain variations. It is not the same during the waking hours and during sleep. During sleep, man eliminates from two to four times less poison than during the period of cerebral activity. The urine of the day and of the night has not the same toxic effects. Thus, while the urine secreted during sleep is convulsivant, that secreted during the waking hours has no such property, but produces narcosis. From this fact a new theory of sleep has been broached,—namely, that this physiological act is determined by the accumulation of toxines producing narcosis.

But let us return to our subject, and apply these data to the study of the *rôle* of the liver as a destroyer of poisons. In a large number of observations, which you will find reported in their entirety in the thesis of Roger,* in which he has studied the toxicity of the urine in patients suffering from hepatic affections, this experimenter has in the immense majority of cases found a notable augmentation of the urotoxic coefficient; this might have been expected, since the suppression of the physiological functions of the liver entails the disappearance of its destructive action on the animal toxines.

But now that this first fact has been determined, we must admit that, from a clinical point of view, it can furnish us but uncertain data, and this is true for numerous reasons: first, because such researches belong much more to the domain of the laboratory than to that of clinical medicine; and if at a recent competitive examination for the election of graduates to fellowship there were placed at the disposal of the competitors a patient, a laboratory, and a hare, it must be conceded that many physicians would be embarrassed if they were obliged in their daily practice to resort to experiments on the hare as a means of diagnosis.

But in admitting even that this fact remains simply an experimental curiosity, it only furnishes information respecting the antiseptic functions of the liver which is absolutely uncertain, because in most diseases of the liver there is icterus and derangement of the biliary secretion. As I shall tell you when I come to speak of the liver as a biliary organ, the bile is much more toxic than the urine, and while it takes forty-five cubic centimetres of urine to kill one kilogramme of living matter, it only takes five cubic centimetres of bile to obtain the same result; the bile is, then, nine times more toxic than the urine. Hence it is that if all the bile were to pass directly into the blood, a man would be poisoned in eight hours and fifty-five minutes; while if the urine of one day were to be reabsorbed, death would not take place in less than two days six hours and thirty-two minutes. Hence, then, according as the bile is secreted or not, the urotoxic coefficient will considerably vary, and for this reason this means of research loses its value if we wish to apply it to the test of the integrity of the antiseptic functions of the liver. Therefore, as you see, we can obtain by the methods which I have just enumerated only hints as to the state of the hepatic cell from the point of view of its destructive action on poisons, and if these hints lack precision, they enable us, nevertheless, to a certain extent, to affirm whether the liver is performing its functions well or badly.

I recommend to you especially the spectroscopic examination of the urine, which enables you to detect the presence of urobilin. And if the result proves negative, you can employ the method of experimental glycosuria, or the test for the toxicity of the urine.

I pass now to the therapeutic conclusions which are derived from the facts which I have just presented.

In diseases of the liver and in infectious diseases, whatever may be their origin, the therapeutist has the greatest interest in maintaining in its integrity the destructive power of the liver on toxines, and he should even

* "On Alimentary Glycosuria" (*Revue de Médecine*, October, 1886).

endeavor to augment it. To attain this there are two orders of means: 1. To augment the glycogenic functions of the liver, since, according to physiological experiments, there is an intimate correlation between the glycogenic functions and the antiseptic functions. 2. To restrain, as far as possible, the sources of poisoning of the organism, so as to reduce to its minimum the quantity of toxines which the liver must destroy.

Let us examine each of these points; and, first of all, the means for augmenting the glycogenic function of the liver.

We know, and I shall insist on this point in another lecture, that the glycogenic functions of the liver are depressed by abstinence and by hyperthermia.

If abstinence, or the withholding of food, be sufficient to diminish glycogenesis, we see the necessity of sustaining fever patients and those that have hepatic diseases by a diet as restorative as possible. It is well understood that this alimentation should fulfil certain conditions, to which I shall return in a moment when I come to the second point of the question which I have to treat,—*i.e.*, the suppression of the causes of the poisoning of the organism; we shall see, in fact, in this connection, that food is one of the most active sources of this poisoning.

To the necessity of feeding the fever patient is joined another necessity,—that of bringing down the temperature. Hence, then, from the exclusive point of view of the antiseptic functions of the liver, we must combat the hyperthermia. You know that we can attain this end by cold baths or tepid baths, or by the employment of that group of medicaments which are derived from the aromatic series. But these medicines have the great disadvantage of diminishing the secretion of urine, and thereby the excretion of toxines.

In his experiments, Roger seems to have demonstrated that glucose by itself modifies the toxicity of the vegetal or organic alkaloids. We may even draw this other conclusion,—that sugar, and glucose in particular, may be utilized advantageously in the infectious diseases, and that it is a good thing to sweeten the drinks of patients that have these affections. But this is a point that needs further study.

But of more practical importance is the great question which I am now to take up,—viz., how to restrain within the narrowest limits the sources of infection of the economy. We may in truth say that in every affection of the liver, of whatever nature, this indication forces itself upon us.

In the previous lecture I showed you that the animal toxines have four sources. They come from foods introduced into the digestive tube; they result from fermentations, which there take place at different stages of the digestive process; they are secreted by the microbes, which abound in the intestinal canal; lastly, they result from the activity of the cell-life.

To these four origins it is necessary to add a fifth, which is of pathological or, it may be, of therapeutic origin. Those of pathological origin are the toxines fabricated by the microbe that causes the infection, and constitute that class of diseases to which I have given the name of toxi-infections. Those of therapeutic origin are the alkaloids which we administer with curative intent.

From this particular point of view, in all patients suffering from affections of the liver, it is necessary to be very chary in the use of alkaloids introduced by the digestive tube, and, in particular, of certain of them such as morphine, atropine, and quinine, and to employ in preference the hypodermic method, because, in accordance with what I have said in a previous lecture, it remains demonstrated that the liver, having a special destructive action on these vegetable alkaloids, when this organ is diseased this destructive action no longer exists; and one is then quite likely to see supervene toxic accidents with therapeutic doses of these alkaloids introduced by the mouth.

As for the other causes of infection which I have above enumerated, we can combat or attenuate them by three orders of means:

1. By the practice of intestinal antisepsis.

2. By favoring the channels for elimination of the toxines.

3. By diminishing cellular activity.

Let us study each of these means.

Intestinal antisepsis, which constitutes the basis of this special medication, includes,—1, antiseptic medicaments; 2, purgatives; 3, a special dietetic regimen. At the head of antiseptic medicaments, we must place salol and salicylate of bismuth. Already, in a former number of the THERAPEUTIC GAZETTE, I have mentioned the reasons which have led me to consider salol as the best intestinal antiseptic. For instance, salol is but little irritating, being always well tolerated by the digestive tube; it is but little soluble, and is

not decomposed into phenic and salicylic acids till it reaches an alkaline medium,—*i.e.*, the intestines. If I add that this salicylate of phenol is relatively but little toxic, I shall have shown the superiority of salol over iodoform and naphthol, hitherto employed, and which are always toxic and irritant.

Almost on the same level I place salicylate of bismuth, which is applicable to the stomach as well as to the intestines, and I prescribe these two medicines under the form of wafers (*cachets médicamenteux*). This is my formula:

℞ Salol,
Salicylate of bismuth,
Bicarbonate of sodium, āā 10 grammes (150 grains). M.
Sig.—Make into thirty medicinal wafers.

As for the naphthols, I have not abandoned their use, but I employ them in lavements, and practise thorough washings of the intestines with a quart of the following solution:

℞ *Alpha*-naphthol, gr. v;
Boiled water, Oii.

I prefer the *alpha*- to the *beta* naphthol because it is more soluble and more active, while being less toxic. I recognize at the same time that the *alpha*-naphthol is more irritant than the *beta*, but, diluted in the proportion of three to five grains to a quart of water, these irritant qualities are extremely feeble, and in the majority of cases this solution is tolerated.

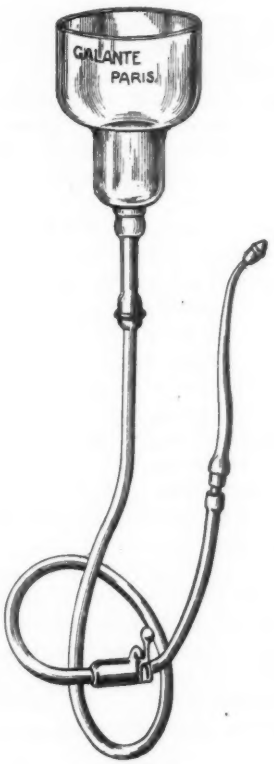
In order to introduce this quantity of liquid, I would advise an apparatus similar to the fountain-syringe, and which I call an *enteroclyseur*, and which is made, according to my directions, by Galante (see Fig. 3). The canula, well oiled, is carried up into the rectum as far as possible, then the liquid is allowed to penetrate slowly and distend the large intestine; the patient, of course, is in a recumbent posture. The liquid which has thus been injected is not to be retained, but should be voided almost immediately.

2. Purgatives are only or principally useful when there exists constipation, and especially when this constipation resists the enemata of naphthol, of which I have just spoken. In such cases, the simple laxatives should be preferred. You can make use of my laxative powder, the formula of which I have many times given, and which I will once more repeat:

℞ Senna-pods,* washed with alcohol, in powder,
Sublimed sulphur, āā 6 grammes;
Pulv. fennel,
Pulv. star-anise, āā 3 grammes;
Pulv. pot. bitart., 2 grammes;
Pulv. licorice, 8 grammes;
Sacch. alb., 25 grammes. M.
Sig.—A heaping teaspoonful, in water.

You can also employ cascara sagrada and its extract cascarine, or the various purgative mineral-waters, and in particular such as come from Spain, which, by reason of the high percentage of saline ingredients which they contain, may be administered in doses of a wineglassful or more in the morning fasting. I might particularize the waters of Rubinat, Carabana, and of Villacabras.

FIG. 3.



Enteroclyser.

Diet plays a preponderant *rôle* in the congeries of therapeutic means designed to effect intestinal antisepsis. Here we witness the triumph of vegetarianism.

Regimen.—The dietetic regimen which I advocate, and which you will find set forth in

* *Vide* THERAPEUTIC GAZETTE, 1889, p. 624.—TR.

its entirety in the recent work of Bonnejoy,* "On Vegetarianism," is composed, as you know, of eggs, milk, cereals, green vegetables, and fruits, and in order that you may always keep in mind the essentials of this regimen, I will here reproduce the directions which I am in the habit of giving:

The patient should live exclusively on milk, eggs, cereals, green vegetables, and fruits.

a. He should drink at least a quart of milk a day.

b. The eggs should be taken in any and every form that pleases the patient.

c. The starchy foods should be in mash or stew (stewed potatoes, beans, lentils, *revalescière racahout*, lactated farina; gruels and puddings of wheat, rice, Indian corn, barley, oatmeal, macaroni, strained panada).

d. The vegetables should be thoroughly cooked,—stewed carrots, turnips, Julienne; cooked salads, stringed beans, green peas, spinach, etc.

e. The fruits should be stewed, with the exception of grapes and strawberries.

If the patient is determined to eat meat, you will allow him only such meats as are well cooked and such as contain principally gelatin, such as lambs' or calves' feet stewed, calves' head, Scotch scollop, the sweet-breads of young calves (*riz de veau*), beef *à la mode*, calves' jelly, etc.

You will forbid fish, game, mollusks, crustaceans, old cheese, by reason of the toxines which putrefaction often develops in these articles of food.

You will also prohibit fat broths, while permitting such soups as are made of lean meats; *bouillon* is forbidden, as the potassa which it contains is prejudicial to the glycogenic functions of the liver, and thereby to its antiseptic functions.

You will permit sweetmeats and preserves, we have, in fact, seen in the previous lecture that sugar aids hepatic antisepsis.

So much for intestinal antisepsis. The second indication to fulfil is to favor elimination of the toxines formed in the organism, and you will attain this end by promoting diuresis by abundant beverages and such as are slightly alkaline, employing in preference white wine or restricting the patient to milk. You will take care, moreover, to promote the functions of the skin by tepid-water lotions, to which you may add Cologne water or soap for those with greasy skins; these lotions are to be followed by energetic frictions with a hair glove.

Lastly, to diminish the leucomaïnes, the result of the organic operations of the cells, you will remember that excessive labor augments in very marked proportions the productions of these toxines. By the words *excessive labor* I mean mental as well as muscular toil. Hence overwork, which is one cause of the accumulation of these toxines, should be absolutely interdicted.

Such is the sum of therapeutic and hygienic means which you can put in operation; it remains for me, in finishing this lecture, to indicate in what cases we can apply this congeries of curative measures.

We have seen that there is a correlation between the glycogenic functions of the liver and its antiseptic functions. When I come to speak of the glycogenic liver, I will endeavor to show you that the integrity of the glycogenic functions is itself the measure of the integrity of the physiological functions. Hence, then, the action of the liver on poisons can only be properly realized when this organ is in a healthy state.

This first conclusion being admitted, there immediately flows therefrom a second,—namely, that all the affections of the liver modify to a greater or less extent its antiseptic action, and when these affections are accompanied by lesions sufficiently profound to destroy the hepatic cell, this antiseptic function will completely disappear. Hence, then, in all affections of the liver, we should employ the means which I have indicated, since this protective barrier established between the intestine and the rest of the economy will be more or less destroyed.

But just as diseases of the liver demand from the physician an antiseptic medication, so also and with more reason, the intestinal affections, and in particular those that are accompanied with putrid fermentations, demand the same therapeutic measures to oppose the affections of the liver consecutive to intestinal putridity. Bouchard, in fact, has shown us that in all patients affected with dilatation of the stomach,—and to these we must add those suffering from dilatation of the large intestine, as Trastour and Malibran have pointed out,—there are intermittent hepatic congestions. These congestions result from exaggeration of this antiseptic activity of the liver, whose functional resources are overtaxed by reason of the excessive production of intestinal toxines.

When you watch day by day these patients

* Docteur Bonnejoy, "Du Végétarisme", avec introduction par Dujardin-Beaumetz. Paris, 1891.

with dilatation of the stomach or intestine, you observe these hepatic congestions as a result either of errors in diet or neglect of the antiseptic medication.

When formerly lecturing on "Dilatation of the Stomach and Gastric Neurasthenia,"* I showed you that this congestion of the liver furnishes an explanation of the renal ectopia so often seen in such patients. Lastly, in infectious diseases with putridity, in order to avoid hepatic complications, it is incumbent on us to resort to the same medication.

Finally, do not forget that if the liver is a destroyer of poisons, it is also a producer of a very active toxine, especially in certain animal species. You are all familiar with poisoning by mollusks; this is produced by a toxalbumen secreted by the liver,—mitylotoxine. When I come to speak of the biliary liver, I shall have something to say about urticaria of hepatic origin, and I hope to show you that the human subject also produces, when the liver has been infected, a toxine similar to mitylotoxine, and which engenders, when the bile which contains it is absorbed by the intestinal mucosa, forms of urticaria of variable intensity. Here also you must trust to intestinal antisepsis such as I have just described for the successful treatment of these urticarias of toxic origin.

The brief considerations which I have just set forth show you the importance of the liver as a destroyer of poisons, and I hope that you have derived hints therefrom which will be of profit to you. In the next lecture I shall speak of *the biliary liver*.

THE SURGERY OF THE SPINE.†

By J. William White, M.D.‡

In endeavoring to systematize the study of the surgery of the spine it has seemed to me well to limit myself to the conditions requiring operative interference, or to those in which operation may reasonably be considered, and to exclude such as lateral curvature, the so-called "railway spine," etc., some of which belong to the province of the orthopædist, others to that of the neurologist.

I may classify those remaining as follows:

A. Congenital deformities;
B. Tuberculosis of the spine;
C. Neoplasms;
D. Traumatisms.

A. Under the first heading, spina bifida is the only condition which is at once of sufficient frequency and sufficient importance to demand special consideration. I have carefully reviewed the entire subject, and see no reason at present to dissent from the conclusion arrived at by the committee of the Clinical Society of London, and held, I believe, by the majority of surgeons at the present day. This is, that while various successes have been reported by other methods, such as simple tapping and drainage, and more recently in a limited number of cases by excision of the sac, yet on the whole the method of injection of the sac offers the best prospect of ultimate recovery with the least immediate danger. The method is described as follows:

> The sac being cleaned, a syringe which will hold about two drachms of an iodo-glycerin solution is chosen, and a fine trocar. The calibre of this must not be too fine for the thick fluid (iodine, 10 grains; iodide of potassium, 1 drachm; glycerin, 1 ounce) which has to pass through it. The puncture in the swelling should be made well at one side, obliquely through healthy skin, and not through the membranous sac-wall, the objects being to avoid wounding the cord or nerves, and also to diminish the risk of leakage of the cerebro-spinal fluid. Unless the sac is very large, it is probably better not to draw off much, if any, of the fluid from the sac on the first occasion. The position of the child during the injection has been a good deal dwelt upon, most authorities recommending that it should be upon its back. The Clinical Society's committee advises that the child should be laid upon its side. About a drachm is the quantity which they recommend. Every care must be taken to prevent any escape of the cerebro-spinal fluid now and later, it being clearly understood that any leakage, which is most difficult to prevent, may lead to septic meningitis and death. When the needle is withdrawn the puncture should be pressed around it, and immediately painted with collodion and iodoform, a dressing of dry gauze being also secured with collodion. A little chloroform may be given, to prevent any crying and straining at the time. The child should be kept as quiet as possible afterwards, on its side, and an assistant should make sure, for the first hour at least, that no leakage is going on. Shrinking of the cyst, setting in rapidly and continuing steadily, shows that all is well. If the injection fail altogether, or only cause partial obliteration of the sac, it should be repeated at intervals of a week or ten days.§

Sinclair, Robson, Evans (*N. Y. Med. Journ.*, 1888, p. 205), Hurd (Therapeutic Gazette, October 1, 1889), and others have reported

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* Therapeutic Gazette, January, 1890, p. 1.

† Read before the American Surgical Association at the Congress of American Physicians and Surgeons, Washington, D. C., September 23, 1891.

‡ Professor of Clinical Surgery in the University of Pennsylvania.

§ W. H. A. Jacobson, "Surgical Operations," Philadelphia, 1889, p. 986.

successful cases of excision, and more recently Bayer (*Prager Med. Woch.*, No. 20, p. 227) has reviewed the whole subject, and rejects the use of the seton, the injection of iodine, and the excision of a part of the sac, as being at the same time unsatisfactory and dangerous. He urges that the condition is one analogous to hernia, and should be treated in a somewhat similar manner; that the danger of meningitis in the one case is no greater than the danger of peritonitis in the other, and that, as compared with the operation above mentioned, it is both safer and more radical. In a child, 10 days old, in whom there was a large meningocele of the size of an apple, and who had already developed bed-sores, he performed the following operation:

The child was chloroformed, and the region of the bed-sores cleaned and rendered aseptic. Two lateral flaps were made from the skin covering the tumor, and were dissected down to its pedicle. The child was turned on its belly in order to avoid excessive loss of cerebro-spinal fluid, and the sac of the meningocele was opened. The cauda equina was seen flattened out on the posterior wall of the sac. It was loosened after dilatation of the incision, and was replaced in the spinal canal. The sac of the meningocele was then removed, leaving only two lateral flaps of the dura, which were sewed together after thorough antiseptic cleansing of the wound. The muscles and skin were afterwards brought together separately. The child recovered completely. Bayer suggests that possibly in the future, through a greater development of the technique of the operation, a bony roof over the sewed sac may be produced by forming two lateral periosteal flaps from the canal of the sacrum.

As I have had no personal experience with this procedure, and am not aware that it has been repeated by other surgeons, I have no opinion to express as to its merits, except to say that if we are to improve upon our present methods, it will be through the establishment on a sound basis of some such ideal operation.

B. In the second group of cases, which includes the various forms of tuberculosis of the spine,* our indications for operative interference may be,—1, the evacuation of pus; 2, the removal of a sequestrum or of the focus of carious bone; 3, the relief of the cord from pressure by pus, bone, or, most commonly, by the products of a simple or tuberculous external pachymeningitis.

These indications may coexist or may be quite distinct.

In considering the removal of the diseased bone, we may speak first of what Lannelongue has called "posterior vertebral disease,"—that is, cases in which the osseous lesions are localized in the transverse processes or in the laminæ. Chipault quotes cases of his own and of others,† including one by Demoulin (*Bull. Soc. Anat.*, 1887, p. 91), in which the removal of fragments of the arches of the twelfth dorsal and first lumbar vertebræ was followed after a brief period by a marked angularity of the spine just at the point of operation, and by complete paraplegia.‡

In all the other cases, seven in number, the result was perfectly satisfactory. Unfortunately, posterior vertebral disease is extremely rare. As regards disease of the bodies of the vertebræ, the most accessible region is without doubt the lumbar, and the now well-known operative procedure of Treves may be adopted as the best, with perhaps the slight modification suggested by Delorme, which consists in cutting the fibres of the quadratus lumborum on the level of each process and then abandoning the further use of cutting instruments.

The height to which we may safely ascend in attacking the laminæ of the vertebræ is a matter of much practical importance. According to Treves, the body of the twelfth dorsal vertebra is accessible, and Chipault admits that this is true provided that the finger is kept close to the spine and carried through the fibres of the psoas. If it is carried laterally he thinks that there is great danger of opening the pleura, which descends on each side below the twelfth rib, often even to the level of the transverse process of the first lumbar. As to the indications for interference, the existence of a psoas abscess, whether its vertebral origin is proved or not, is sufficient warrant for the lumbar operation, as affording the best drainage and at the same time permitting the removal of a sequestrum or the curetting of a patch of superficial caries, if they should be discovered by the finger or the probe.

Boeckel was the first to make a posterior

* In relation to this, as to the surgery of the spine in general, I desire to acknowledge my indebtedness to the recent and valuable additions to the literature of this subject made by Chipault, whose careful and exhaustive review of its history (*Archives Générales de Médecine*, October, November, and December, 1890) leaves little to be done in this direction.

† Lannelongue (*Bull. Soc. Chir.*, 1878, p. 162), Boeckel ("Fragments de Chirurgie Antiseptique," Paris, 1882, ch. v.), Polaillon (*Union Médicale*, 1883, t. i. p. 1073), Reclus (*Critique et Clinique Chirurgicales*, 1884, p. 114), Delorme (*Bull. et Mém. Soc. Chir*, November, 1886, and *Archiv. de Méd. et de Pharmacie Militaires*, 1887, t. i. p. 51).

‡ Chipault does not believe, however, that the surgical intervention should be regarded as the cause of this accident.

opening for an inguinal abscess of vertebral origin, Treves has operated twice, Fraenkel once, all of them successfully. If there should be any doubt as to this form of interference in cases where the abscess points in the inguinal region, there certainly can be none when it shows itself in the loin. Cases in which, after evacuation of such an abscess, diseased bone from the vertebra has been successfully removed have been quite frequent. Delorme, Buffet, Reclus, Abbé, Saint-Germain, Tuffier, and others have done this with good results. Sometimes the artificial cavity formed in the psoas renders accessible portions of the spinal canal which could not otherwise be safely approached. Delorme has in this way been able, guided by a lumbar abscess and after removing a portion of the twelfth rib, to curette the body of the eleventh dorsal vertebra. In these cases the pleura is protected by the wall of the abscess, and is probably itself thickened by a pleuritis in the vicinity.

The success obtained in the lumbar region has led several surgeons to attempt to operate upon the bodies of dorsal vertebræ.

Boeckel, having failed to cure an abscess situated at the inferior angle of the scapula by incision, curetting, and disinfection, enlarged his incision, removed a portion of carious third rib, and curetted the lateral aspect of the bodies of the second and third dorsal vertebræ. The patient recovered. Grünbaum did the same operation on the tenth dorsal.

Chipault believes, and, I think, correctly, that while the dorsal vertebræ are not absolutely inaccessible, and while in some cases they *may* be reached and curetted, such cases must be very exceptional, and it must be added that we have no satisfactory evidence that the vertebral bodies themselves have been attacked in such cases as the above. Where the disease is associated with caries of one or more ribs the processes are more likely to be affected, and are in all probability the portions reached by the finger and the curette of the surgeon. Dr. Agnew and I have had one such case, in which caries of the vertebral extremity of the third rib was the cause of an enormous cold abscess occupying the whole of the right scapular region, and in which we curetted the process, *not* the body of the corresponding vertebra.

Three principal objections have been made to attempts to reach the focus of bone-disease in these cases of spinal abscess:

1. It is said that operative methods practicable upon the cadaver cannot be employed in the presence of the angular deformity of Pott's disease, in which the last rib may be in actual contact with the ilium. These cases, however, are rare, and in them there is little indication for surgical interference, as the bodies of many vertebræ are deeply affected. The favorable cases are those of superficial caries, in which there is a curve of very large radius, or those in which there is disease of but a single vertebra and a very slight angular projection. In these two classes of cases the costo-iliac space is but little reduced.

2. It is said that the solidity of the spine is affected by operation. This, of course, cannot apply to cases in which curetting is employed. It is scarcely possible that the removal of a sequestrum could materially affect the integrity of the spine; and indeed this has not occurred even after very extensive removal of portions of vertebræ.

3. Operation is said to be useless. As a matter of fact, we have now the records of fourteen operations upon the bodies of vertebræ, with eight cures, five cases improved, and one death, which had no relation to the operation itself. From these facts the following conclusions seem justifiable:*

> The search for the focus of bony disease in caries of the spine is indicated with sufficient frequency to give the matter a real surgical interest. The posterior arch is accessible at all levels. The seat of disease may be sought in exceptional cases even before the appearance of an abscess, and in the presence of an abscess may be reached with great ease and certainty. The bodies of the lumbar vertebræ are accessible by Treves's operation even when no abscess which is clinically appreciable is found. If an abscess exists, even when it points anteriorly, it is proper to open it by the lumbar route and seek the focus of disease. If it points in the loin, it should, of course, be opened in that region. The lumbar incision permits us, if the abscess depends on vertebral disease, to reach, without wounding the pleura, the twelfth dorsal vertebra, and even occasionally to go above it. The eleventh dorsal is usually, in my opinion, not to be reached with safety, and, unless extensive caries of the ribs has taken place, the bodies of the other dorsal vertebræ are not to be approached surgically even in the presence of an abscess.

As to the third indication in tuberculosis of the spine,—viz., the relief of cord-pressure in Pott's paralysis,—we have some encouragement to operation held out in the clinical facts:

1. That there are very many examples of the relief of paralysis in spinal caries after the pointing of a psoas or an iliac abscess, or after the evacuation of pus by the side of the spine. It is scarcely worth while to give special instances of the truth of this statement. It is familiar to all who have seen many cases of

* They differ somewhat, but not materially, from those reached by Chipault.

Pott's disease. Of course in such cases the evacuation of the abscess when it is accessible, or the performance of Treves's operation for the removal of the carious portion of the vertebræ, is clearly indicated.

2. That the disease of the dura mater in Pott's disease is limited to the site of the diseased vertebræ. The change from an inflammatory to a normal area is an abrupt one. Dr. George R. Elliott, in a valuable paper on the "Pressure Paralysis of Pott's Disease" (*N. Y. Medical Journal*, June 2, 1888), has fully developed this point, long ago advanced by others, in an argument to sustain the theory that this paralysis is due chiefly, almost entirely, to pressure, and not usually to grave changes in the cord itself. Some of his conclusions, sustained by autopsies in twenty-one cases, have a direct bearing upon the question and may be found subjoined.*

That these statements, at least so far as they attribute the paralysis of Pott's disease to an external pachymeningitis rather than to direct pressure from the bones themselves, are not new ones, a few quotations will demonstrate. Hippocrates, writing of spinal cases, said,—

"The palsy of the legs and arms, the wasting of the body, and the retention of urine happen chiefly in those cases where there is no spinal deformity, nor any projecting bone forward or backward; and paralytic affections take place the least frequently in cases of gibbous or angular projections."

Mr. Percival Pott, writing in 1720 on "The Useless State of the Lower Limbs in Consequence of a Curvature of the Spine," says,—

"Since I had been particularly attentive to the disorder, I thought that I had observed that neither the extent nor degree of the curve had in general produced any material difference in the symptoms, but that the smallest was, when perfectly formed, attended with the same consequences as the largest, and that the useless state of the limbs is by no means a consequence of the altered figure of the spine, or of the disposition of the bones with regard to each other, but merely of the caries."

Michaud, in 1871, completely elucidated the mechanism of this form of paralysis ("Sur la Méningite et le Myélite dans le Mal vertébral"), and Buzzard (*Transactions of the Clinical Society of London*, vol. xiii., 1880), Gowers ("Diseases of the Nervous System," 1886, vol. i. p. 169), and others have, by autopsies and carefully reported cases, confirmed these views.†

On May 9, 1883, Mr. William Macewen, in a case of complete sensory and motor paraplegia, with incontinence of urine and fæces, and of two years' duration, the result of angular curvature of the spine, removed the laminæ of the fifth, sixth, and seventh dorsal vertebræ. Between the theca and the bone he found a fibrous neoplasm one-eighth of an inch in thickness firmly attached to about two-thirds of the circumference of the membranes; this was carefully dissected off; twenty-four hours later improvement began, and in six months the patient was able to walk. In five years afterwards he was attending school and joining in all the games, including foot-ball. Mr. Macewen's cases of paraplegia in Pott's disease included four others, two of which were successful, one of them being of the most aggravated character imaginable. A third case was also successful. The two fatal cases died, one a week after the operation, the other eight months later, of general tuberculosis.

Recently Mr. Horsley has operated upon a case of complete paralysis of all four limbs from caries of the second and third cervical vertebræ, and reported the case as rapidly regaining power. The wound healed without the slightest complication.

In December, 1888, I operated upon a paraplegic patient with Pott's disease and angular curvature. The patient had grave visceral lesions, which, together with the shock of the operation, caused his death. The autopsy disclosed the existence of tubercular disease of both lungs; amyloid kidneys; a large tubercular mass undergoing softening, and connected with the bodies of most of the dorsal

* It seems to him demonstrated: That we have present a simple mechanical pressure in the form of abscess products, thickened dura or bone; that the inflammatory process is invariably a limited one, the inflammation of the dura mater being restricted exactly to the site of the diseased vertebræ, exhibiting no tendency to extend in the membrane,—nature's medullary protection; that the medullary surface of the dura mater is almost invariably normal. It follows, then, that the pressure lesion is simply mechanical, possessing no tendency to involve the spinal cord through any inherent specific characters of the carious process—a bland mechanical lesion—and the damage it inflicts is commensurate with the pressure exerted.

Experimental physiology gives no evidence of an inflammatory lesion following experimental compression of the cord. A careful examination of the pathological findings of reported cases reveals to us, in the light of recent pathological knowledge, but few instances where we have reason to believe the original lesion to have been inflammatory. These were cases where the process was virulent, leading to a perforation of the dura mater, thus allowing the purulent products to come in direct contact with the cord, giving rise to a lepto-meningitis.

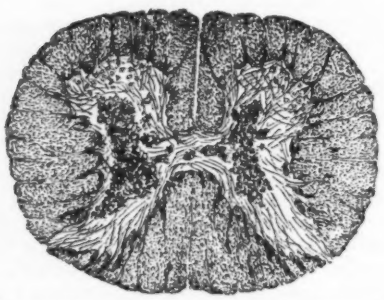
Finally, then, Dr. Elliott concludes, our researches have led to the belief that the original lesion is, as a rule, non-inflammatory, an opinion supported by pathology and experimental physiology, and corroborated by clinical manifestations.

† "Pachymeningitis, with or without suppuration, is generally due to traumatism or vertebral caries, but occasionally cases appear to arise as primary or idiopathic. In this so-called idiopathic suppuration in the spinal dura mater, trephining low down to give exit to the pus might in rare cases prove a useful procedure. Such a case is that reported by Dr. Robert Maguire (*Lancet*, July 7, 1888). Other cases have been reported by Mueller, Spencer (*Lancet*, June 14, 1879), and others." (Charles K. Mills, "Spinal Localization," THERAPEUTIC GAZETTE, May 15 and June 15, 1889.)

vertebræ and with a patch of softened lung-tissue. The cord was normal, except at the level of the eleventh dorsal vertebra, where it was soft. The dura and peri-dural tissues were normal.

Microscopical Examination of Cord.—This proved to be exceedingly interesting, especially in view of the conclusions of many surgeons and neurologists formulated by Elliott, who argue against the occurrence, or at least the frequent occurrence, of inflammation of the cord as a result of compression in Pott's disease. In the first place, the cord showed no alteration in shape, nor did there appear to be any change in the pia mater. A very striking

FIG. 1.



Trans-section of lumbar cord, showing hemorrhagic infarct ("Annals of Surgery"). (Drawn by Allen J. Smith.)

change, however, existed in the gray matter. Here extensive hemorrhagic infiltration had taken place. It was marked on both sides, but especially so on the left; nor was it absolutely confined to the gray matter; here and there the white matter had been invaded. It was of interest to note that this condition was relatively recent. The stage was still that of *red* softening, and little, if any, change had taken place in the blood-corpuscles. There were no indications of long-standing and chronic inflammatory changes. In the cornua of the gray matter the nerve cells were quite well preserved; the nerve-tubules of the white matter likewise presented a normal appearance, and there was no noticeable increase in the neuroglia. Further, it was most marked in sections made from the upper portion of the fragment,—namely, that which had undergone most compression.

I may quote my remarks published at the time the case was reported:

"The result was a surprise and great disappointment to me. So little interference with both bony and nervous structures had occurred; the patient was so young; his general appearance was so good, and his nutrition so excellent, that my prognosis, based also on the reports of the attending physicians and neurologists, was very favorable, as regarded the immediate effects of the operation. The autopsy, of course, disclosed a condition, or rather a series of conditions, that went far both to explain the fatal result and to render it less distressing, as the case was evidently a hopeless one. Either the spinal abscess or the pulmonary tubercle would have strongly contraindicated operation had their existence been revealed by persistent fever or sweats, by cough or emaciation, or by physical signs. In the absence, however, of these, and of all other significant symptoms, and in the presence of a spinal deformity which seemed to point strongly to a pressure paraplegia, the operation seemed justifiable.

"The immediate cause of death, I am disposed to believe, was a combination of shock with the free use of ether in a patient with crippled lung-power, a degenerated heart-muscle, and amyloid kidneys. There was no hemorrhage of any moment, and indeed the quantity of ether administered was not excessive as compared with that used in every-day operations on ordinary patients.

"The chief lessons of the case are: 1. The apparently unavoidable risk due to insidious and unrecognizable complications, a risk which is probably greatly increased in tuberculous patients. 2. The existence of red softening in a case of Pott's paralysis, a condition which, if we can judge by recorded autopsies and by the expressed opinion of many competent observers, is very exceptional. 3. The necessity for extreme caution in the use of anæsthetics in all cases in which the existing and recognized disease makes coincident visceral changes probable, or even possible."

Dr. Robert Abbé, of New York, has (*Medical Record*, February 9, 1889) reported a case which he describes as an extra-dural tumor of the spine, but which seems to have been a case of caries of the dorsal vertebræ followed by localized suppuration with inflammatory thickening and desiccation of pus, rather than a tumor, causing caries by pressure. In this case the spines and laminæ of the eighth, ninth, and tenth dorsal vertebræ were cut away by the rongeur, and the mass of dense tissue above the spine removed. The patient made a rapid recovery, regaining the ability to walk while pushing a chair. In another case of intractable brachial neuralgia the laminæ of the vertebræ from the fifth cervical to the third dorsal were removed on the right side, and the posterior roots of the sixth, seventh, and eighth cervical nerves were divided. The patient made a quick recovery.

Mr. Duncan has reported (*Edinburgh Medical Journal*, March, 1889) a case of paraplegia of nearly a year's standing, and due to Pott's disease, relieved by the removal of the spines and laminæ of the fourth, fifth, sixth, and seventh dorsal vertebræ.

Mr. W. Arbuthnot Lane reports (*British Medical Journal*, April 20, 1889) a case of Pott's disease with angular curvature in a child aged 7½ years. The curvature was about the middle of the dorsal region. For eleven months he had been paralyzed. The spinous processes and laminæ of the fourth, fifth, and sixth dorsal vertebræ were removed with bone forceps. The body of the fifth appeared abnormally near the laminæ of the adjoining vertebræ, and the cord seemed to have been forcibly compressed between those bony points. The wound healed under two dressings. A month later he was able to move both legs freely, and at the time the case was reported seemed on the road to recovery.

The cases of Macewen give strong corroborative evidence in favor of the theory of extradural pressure rather than interstitial cord change as the cause of the paraplegia in these cases, as does that of Horsley, the cord recovering there from the effects of the pressure of the tumor, which had been exerted with sufficient firmness, and for so long a period, as almost to hemisect it. Duncan's case and Horsley's other case (of Pott's paralysis) are also valuable evidence in the same direction.

Resection of portions of the body of a vertebra has been performed in paralytic cases on the sixth and seventh cervical vertebræ by Podres (*Russkaja Med.*, No. 19, 1886), with recovery; on the dorsal vertebræ by Boeckel, with excellent results (*Schmidt's Jahrbücher; Phila. Med. Times*, September 8, 1883). James Israel (*Berliner Klin. Wochenschrift*, March 6, 1882) resected the arch and body of the last dorsal vertebra for caries and spinal abscess. The patient died of coincident tubercular disease on the thirty-seventh day.

Dr. John A. Wyeth has reported a case of a boy 9 years of age with Pott's disease affecting the mid-dorsal region, with paresis of the bladder and complete paraplegia of the voluntary muscles of the lower extremities. Sensation was not disturbed. A plaster jacket was applied while the patient was suspended. Four months after his admission there was no improvement. As a result of catheterization to relieve retention of urine a severe urethritis and cystitis ensued, for which supra-pubic cystotomy for drainage was done. Six months after admission, as it had been determined to explore the vertebral column, the spines and laminæ of the third, fourth, fifth, sixth, and seventh dorsal vertebræ were exposed by a median single incision, and the skin and muscles were lifted together from the bones. The laminæ of the fourth, fifth, and sixth vertebræ were divided on each side, and, with their spines, were removed. The dura was very tense immediately beneath the right lamina of the fifth vertebra, and bulged out well beyond the level of the bone which had just been removed. On opening the dura, a sharp prominence occupying the right half of the posterior portion of the cord was observed. It was about half an inch in perpendicular measurement. On puncture, a creamy substance escaped, to all appearance like thick pus. About thirty minims were evacuated, and the tumefaction was entirely relieved. The dura was closed with fine catgut sutures, and a moist aseptic dressing was applied. The patient made a good recovery, but up to five weeks after the operation there had been no improvement in the paralysis. The bladder-drainage was being continued. The fluid from the abscess consisted chiefly of broken-down cord substance, with a few leucocytes. The case was evidently one of compression of the cord from Pott's disease (*New York Medical Journal*, 1890, vol. li. 1, p. 271).

Dr. Wyeth reported later that his patient was able to sit upon the side of the bed and make some steps. At the same meeting Dr. Abbé reported the case of a young adult having complete paraplegia due to a similar neoplasm compressing the cord. Three weeks after operation the patient began to walk, and was finally cured (*New York Medical Journal*, 1890, vol. li. 1, p. 271).

Mr. Henry Thompson reports a case of recent paraplegia following caries of the vertebræ at the cervico-dorsal junction. The paralysis was very extensive, extending up to the arms, and the patient was steadily deteriorating. The spines and laminæ of the last cervical and the first and second dorsal vertebræ were removed. The dura mater at first looked compressed and did not pulsate, but in a few minutes it seemed to do so. There was no appearance of any cicatricial or inflammatory tissue, but the bodies of the affected vertebræ seemed to be compressing the cord. It was not thought necessary to open the sheath, so the wound was stitched up, covered over with protective and a thick layer of boracic acid powder, and upon that a layer of alembroth wool was placed. A small drainage-tube was inserted before the wound was sutured. For some days he was very ill, although the tympanites almost immediately began to disappear; but after he had got over the shock of the operation he gradually but steadily improved. At the time of the report he had so greatly improved that it was thought he would entirely recover (*London Lancet*, 1889, ii. pp. 315, 316).

In July, 1888, Dr. John B. Deaver operated upon a patient of Dr. Hendrie Lloyd's who had a swelling involving the left lateral region of the cervical spine, which was thought to be a tumor extending outward from the spinal canal. She had paresis of the left arm, paralysis of the left leg, exaggerated reflexes, etc. There was no paræsthesia and no anæsthesia. At the operation the laminæ of the third and fourth cervical vertebræ were cut through, but nothing abnormal in the cord or membranes was found. The cord was explored with a needle. During the latter part of the operation the respiration altered, and continued to be gasping and irregular at intervals until her death, three days later. At the autopsy there was found an old extravasation of blood, with advanced degeneration, a short distance below the decussation of the pyramidal tracts. Death was thought to be due to phrenic inhibition the result of the exploration (*Amer. Journ. of the Med. Sci.*, December, 1888, p. 564).

Mr. G. A. Wright reports a case of a child 7 years of age with Pott's paralysis. The angular curvature was most marked in the mid-dorsal region. An incision about four inches in length was made along the line of the most prominent spinous processes, and the soft parts on each side were separated so as to expose the osseous surfaces. Three laminæ were divided on each side, and were removed with their attached spines, uncovering the theca of the spinal cord, which, at the lower part exposed, was found surrounded by a buff-colored, tough, leathery substance; this was cut away with scissors. The cord did not appear to pulsate, but no point of constriction could be found. The muscles were brought together by deep sutures of catgut, and the skin with waxed silk; a small drainage-tube was left in. The wound, having been dusted with iodoform and boracic acid in equal parts, was dressed with sublimated wool wadding. Careful antiseptic precautions were observed before and during the operation. The trunk was supported by a special iron splint. The wound healed rapidly by first intention, except at the drainage-opening, which, however, also quickly closed; very slight improvement was soon followed by relapses (*Lancet*, 1888, ii. pp. 64–66).

M. Buffet reports a case of tuberculous ostitis of the lumbar vertebræ in which he divided the articular processes and the sides of the bodies,—effecting a

permanent cure (*Gazette des Hôpitaux*, December 2, 1886).

William Thorburn reports a case of operation for paralysis of Pott's disease in a child 6 years of age. Chloroform having been given, a vertical incision, about three inches long, was made in the middle line, over the affected spinous processes; the soft parts were raised from the bones by a raspatory, and the laminæ of the third dorsal vertebra cut through by a Hey's saw and bone-forceps. An opening about the size of a sixpence was thus made in the spinal canal, and about fifteen minims of pus at once escaped. At the bottom of the opening was seen the apparently healthy dura mater. A drainage-tube was then inserted, so bent that its convexity lay in the opening into the spinal canal, and the wound was sutured. The spray was used, with carbolized dressings.

After the operation the breathing was distinctly better, but no other change could be observed. On the following morning the improvement appeared to be maintained, but there was a good deal of pus in the urine. The wound being dressed, no more pus escaped. On the afternoon of this day breathing again became much worse; the temperature, which had fallen to normal, rose to 104.6°, and shortly after midnight the boy died asphyxiated. No post-mortem examination was permitted (*British Medical Journal*, 1888, ii. pp. 665, 666).

Eugene Boeckel reports a case of a woman 43 years of age, in whom an abscess formed beneath the angle of the left scapula, which, after operation, left a fistulous tract some inches in length, and extending upward. This was finally found to lead to the lateral aspect of the bodies of the second and third dorsal vertebræ, which were roughened and carious. These were curetted, drainage-tubes were employed, and at the time the case was reported the patient appeared to be on the road to recovery. Boeckel thinks that in cases of cold abscess connected with the spine we should be content with simple evacuation, but if through the incision we can reach the body of the carious vertebra, it is proper to employ the curette, so as to prepare it for the application of iodoform and antiseptic dressings (*Gazette Hebdomadaire*, 1882, 2d series, vol. xix. p. 171).

Dr. Banham and Mr. Arthur Jackson report the case of a healthy-looking boy, aged 12, who had been completely paraplegic for nine months previous to operation. There was no tubercular history in the family. There was a prominence on the back corresponding to the lower dorsal region. There was much diminution of sensibility. His feet were blue and cold. His legs were drawn up in a state of tonic contraction. He had no control over his sphincters, and there was an exaggerated tendon reflex on both sides, with ankle-clonus. An incision three inches and a half long was made over the lower dorsal spines. The laminæ and spinous process of a vertebra (the ninth) were removed, and the dura mater laid bare, but not opened. No pus was found, but the spinal cord rose to the opening made in the bone. The day after the operation the temperature rose to 102° F., but fell during the next two days to 100° F., where it remained for three weeks, with slight nocturnal elevations; after that it was normal. The wound was dressed for the first time on December 26, twelve days after the operation, and the sutures were removed. On January 10, fifteen days after the last dressing, the wound was again examined, and was found healed. A week after the operation the boy was able, for the first time since his admission, to micturate properly, and when he desired; he had control over his sphincters, for which result alone the operation had been of great value. The painful tonic contraction of his leg- and thigh-muscles had quite disappeared, and he was able to draw his knees up against his abdomen, and slightly move his toes. Faradic contractility was, however, much diminished, though sensibility remained. He could distinguish with accuracy the point pricked with a pin at any part over each leg. His sensations also to touch, pressure, and temperature were normal (*British Medical Journal*, 1883, i. p. 812).

Drs. Bullard and Burrell report the case of a man with paraplegia, bed-sores, girdle-pains, etc., and with a distinct prominence involving the fourth, fifth, and sixth dorsal vertebræ. His condition dated back about seven months. Resection of the third, fourth, fifth, and sixth dorsal vertebræ was accomplished, the spines and laminæ of these vertebræ being turned up, hinging at a divided point of the spine of the second dorsal vertebra. The patient died from shock in thirty-six hours (*Trans. Am. Orthopædic Association*, vol. ii. p. 247).

Jackson (*British Medical Journal*, April 28, 1883, p. 812) removed the laminæ and spinous process of the ninth dorsal vertebra in a case of Pott's paralysis in a child 12 years of age. There was marked improvement afterwards in both motion and sensation. Southam (*British Medical Journal*, September 22, 1888, ii. p 665) operated on a child 6 years of age, and removed the laminæ and the spine of the third dorsal vertebra. A few drops of pus escaped from the canal, and the dura mater appeared thickened. The child died the same evening. Demons (*Bull. Soc. Anat. et Phys.*, Bordeaux, 1888, lix. p. 288), in a man 35 years of age, with complete paraplegia, removed the spines and laminæ of the third, fourth, and fifth dorsal vertebræ. Temporary improvement was followed by death one month later. At the autopsy it was found that the cord was compressed to a much greater extent than had been seen at the time of operation. W. A. Lane (*Lancet*, July 5, 1890, ii. p. 11) reports a case of a man 32 years of age, with dorsal angular curvature and rapidly-developing paraplegia. The spinous processes and laminæ of the ninth, tenth, and eleventh dorsal vertebræ were removed. A mass of granulation-tissue intermingled with pus projected from the canal. This was removed, as well as a slight prolongation which extended to the right and in front of the dura mater. There was complete return of both motion and sensation.

Kraske has reported (Chipault, *loc. supra cit.*) four cases in which he has operated for Pott's paralysis. In all of them there were growths consisting largely of granulation-tissue, and more or less adherent to the dura mater, which were removed by forceps and scissors. In three cases this tissue was infiltrated with pus. In the fourth case it enclosed an abscess containing about two teaspoonfuls of pus, and several sequestra were present. In one case there was no improvement. The patient died eight weeks later from the progress of the disease. Extensive pachymeningitis was found reaching beyond the area of operation. In the others the results were as follows: In one, a man 57 years of age, there was caries of the fifth dorsal arch. The third, fourth, fifth, and sixth arches were resected. Sensibility returned in the limbs that afternoon, motion three or four days later. In one month the patient left his bed and tried to walk; in two months the paraplegia returned, and five months later the patient died of pulmonary tuberculosis. The autopsy showed that the caries had extended to the body of the fifth dorsal vertebra. There was no deformity at

the level of the operation, and at that point the lesions of pachymeningitis were completely cured. In another case, a patient 14 years of age, complete paraplegia, with vesical paresis, was present. The arches of the fifth and sixth dorsal vertebræ were resected. Some hours afterwards motion of the limbs occurred, but two months later the paraplegia returned. At the time of report the patient was still living, but unimproved. In the last case, a boy 12 years of age, with complete motor and sensory paralysis and vesical paresis, the arches of the eighth, ninth, and tenth dorsal vertebræ were resected. Eight days afterwards motion began in the limbs, the bladder acted normally. One month later, however, he became worse, and five months later there was slight motion of the limbs, but he was otherwise unimproved.

Gerster (*Medical Record*, July 26, 1890, p. 131) reports the case of a boy 15 years of age, with paraplegia from Pott's disease. The laminæ of the sixth and seventh dorsal vertebræ were raised and an extensive abscess evacuated. Caseous masses were found encircling the motor and sensory roots on both sides, the transverse processes of the sixth, seventh, and eighth dorsal vertebræ on the right and of the fifth, sixth, and seventh on the left were carious, and the corresponding costo-vertebral articulations were destroyed. All these diseased parts were raised and the bodies of the sixth and seventh dorsal vertebræ curetted. Seven months later the patient was completely cured.

Horsley reported six cases of trephining in Pott's disease at the International Congress in Berlin in 1890: 1. A man, 62 years of age, complete paraplegia for twenty months; trephining; a little suppuration noticed; death from compression; a large abscess found. 2. A woman, 23 years of age; paraplegia for five years; after operation slight improvement in motion and sensation. 3. A boy, 16 years of age; paraplegic for eighteen months; abscess of bone found; union by first intention; slight improvement. 4. A boy, 17 years of age; paraplegic for eight months; union by first intention; very slow improvement in motion. 5. A girl, 15 years of age; paraplegic for one year; bony abscess found; union by first intention; return of sensation in ten days; gradual return of motion; could walk in one month. 6. A boy, 16 years of age; paraplegia; weakness of the left arm dating for three years; union by first intention; patient *in statu quo*.

Chipault (*op. cit.*) reports two cases: One a boy, 9 years of age; complete paraplegia; removal of the fifth, sixth, seventh, and eighth dorsal arches; slight improvement, followed by return of paraplegia. Second case, a boy, 9 years of age; case of fracture occurring during Pott's disease, and followed by sudden paraplegia; resection of the fourth, fifth, sixth, seventh, and eighth dorsal arches; marked improvement in motion, but death from pulmonary disease in three weeks. At the autopsy compression of the cord by a portion of the body of the eighth dorsal, and extensive pachymeningitis were found; death occurred from generalized caseous broncho-pneumonia.*

The analysis of forty cases of this operation for Pott's paralysis, which are all I have been able to bring together, reveals the following facts. In twenty-two there was either improvement or absolute cure. The unsuccessful cases which recovered from the operation were in some instances the subject of secondary disease. Wright's case improved for a month, and three of those of Kraske did likewise, and then relapsed. In the case of Demons, and in one of those of Kraske, the result was absolutely negative. This was due, as the reports show, to insufficient removal of the compressing cause. The deaths were twelve in number, showing a mortality of thirty per cent., and were due to various causes: in my own case, to shock and extensive renal and pulmonary disease; in the case of Deaver and Lloyd, either to the anæsthetic or, as they supposed, to wounding of the medullary centre of the phrenic during the operation; in the cases of Macewen and Coleman and of Chipault, to pulmonary tuberculosis; in the case of Burrell, probably to shock. In the others death was directly due to the gravity of the disease of the cord.†

In coming to a conclusion as to the indications for operative interference in Pott's paralysis, the age of the patient is of great importance, the proportion of cures and of improvements being much greater in children and in adolescents than in adults. Among the twelve fatal cases the age was given in nine, and there were two children and seven adults. Among three cases in which no improvement followed operation: children,—Wright, 7 years; Kraske, 14 years; Hardy, 16 years.

Among twenty-two cases of improvement or cure there were fifteen children and four adults, the age of three being unknown.

While surgical interference is thus shown to be much better borne by children than by adults in Pott's disease, as in most other conditions, we must remember also the well-known clinical fact that the disease itself in children is far less grave and threatening than in adults.

The seat and extent of the osseous lesion are also important, the prognosis being unfavorable in direct proportion to the height of the caries, above all when respiratory complications exist previous to the operation. All the patients who died had an upper dorsal or a

* Selecting from the thirty-five cases collected by Chipault those in which all the details as to the number of arches raised were given, they were as follows: One arch in two (Southam, Jackson); two arches in three (Horsley, Deaver, Kraske); three arches in twelve (Macewen, Lane (2), Wyeth, Wright, Abbé, White, Thompson, Demons, Kraske (3); four arches in two (Buzzard, Duncan); five arches in one (White); unknown in six (Macewen (4), Abbé, Coleman).

† Chipault agrees in a general way with the opinions of Kraske, Bullard and Burrell, and Thorburn, and believes that we should not operate except where we have grave medullary symptoms on the one hand and a fairly good general condition on the other.

cervical lesion except my own, in which case the equally fatal condition of tuberculosis of a large number of vertebral bodies existed.

The effect of suspension in the treatment of Pott's paralysis has been so favorable in a number of cases that it should occupy a prominent position in the consideration of our therapeutic resources, and should always be tried, or, at least, carefully discussed, before operative measures are thought of.

Weir Mitchell (*Amer. Journal of the Med. Sciences*, May, 1889), has, it appears to me, shown conclusively: That suspension should be used early in Pott's disease; that used with care it enables us slowly to lessen the curve; that in these cases there must be in some form a replacement of the crumbled tissues; that no case of Pott's paralysis ought to be considered desperate without its trial; that suspension has succeeded after failures of other accepted methods; that the pull probably acts more or less directly on the cord itself, and that the gain is not explicable merely by obvious effects on the bony curve;* that the methods of extension to be used in these cases may be very various, only provided we do get active extension; that the plan and the length of time of extension must be made to conform to the needs, endurance, and sensation of the individual cases.

I desire merely to express my concurrence with these views, and shall not further pursue this branch of the subject.

The evidence at present available in relation to the operative treatment of spinal tuberculosis with symptoms of pressure upon the cord appears to me to justify the following conclusions:

1. The paralysis in Pott's disease is not, as a rule, due to a transverse myelitis or a hopeless degeneration, and is not usually due to the pressure of the carious or displaced vertebræ, but is, in the majority of cases, the result of an external pachymeningitis, which results in the formation of an extra-dural connective-tissue tumor.

2. Speaking generally, a favorable prognosis is to be given, especially in children, in cases of Pott's paralysis in which the abscess, if any exists, can be evacuated; the treatment by extension and with a plaster jacket can be employed, and the patient can be put under the most favorable hygienic conditions.

3. In cases in which all this has been tried unsuccessfully, or in those in which the disease is slowly but steadily progressing to an unfavorable termination; when with more or less complete loss of motion and sensation below the level of the lesion there are incontinence of urine and fæces and the development of bed-sores, and especially when acute symptoms threaten life, resection becomes entirely justifiable.

4. Operation having been decided upon for any or all of the above reasons, the prognosis will be favorable in direct proportion to the youth and strength of the patient, the absence of generalized tuberculosis, and the nearness of the lesion to the base of the spine.

5. When the tuberculous process affects the arches and there is paraplegia, we may sometimes operate, hoping not only to free the cord, but to remove at the same time the focus of disease. This double indication may also be fulfilled in those cases where, without bony disease, there is posterior pachymeningitis or a tuberculoma occupying the canal. Here again, however, time and careful attention to hygiene, including change to sea- or mountain-air, often work wonders.

6. If the lesion of the bodies of the vertebræ is in the lumbar region at a point where these bodies are accessible, it might be possible in certain cases to expose the cord from the back, by removal of the laminæ, with the object not only of removing pressure but of reaching and taking away the diseased bone and tubercular granulation.

7. In tuberculosis of the body of a vertebra and compression of the cord by anterior pachymeningitis we can fulfil only one indication,—liberate the cord from pressure. We should operate only in grave cases where acute compression, the appearance of respiratory complications, the rapid development of degenerative processes, force us to interfere, or where the course of a chronic case is steadily towards a fatal termination although no advanced visceral tuberculous lesions are present.†

C. Passing now to the third group of cases, and taking up the surgery of the neoplasms of the cord unassociated with traumatism or caries, we may very briefly review the history.

In fifty cases of spinal tumor tabulated by Drs. Mills and Lloyd, and among fifty cases tabulated by Horsley, the two including practically all those of which we have any satisfactory knowledge from the surgical and pathological stand-points, no mention is made of operation in a single instance.

In 1882, Mr. Byron Bramwell, of Edinburgh, in his work on "Diseases of the Spinal Cord," made the following important suggestion in regard to this subject:

* Dr. G. F. Jydston suggests ("Addresses and Essays," Chicago, 1891) that the elongation of the spinal canal produces a vacuum, which causes an increased vascular supply to the cord with a corresponding increase in its nutrition.

† Chipault, *op. cit.*

"The removal of extra-medullary tumors is not yet a recognized plan of treatment. This is partly owing to the uncertainty of diagnosis, and partly to the serious nature of the operation. The difficulties in diagnosis are disappearing, and, thanks to antiseptic surgery, we can now conduct operations which were formerly unjustifiable. I would advise an operation in any case in which the symptoms were original, in which the diagnosis clearly indicated the presence of a tumor, in which there was no evidence of malignant disease, in which the exact position of the growth could be localized, and in which a vigorous anti-syphilitic treatment had failed to produce beneficial results."

Other neurologists, notably Mr. Gowers, expressed the same general views; but the strong prejudice existing among surgeons against the operation of resection, and the uncertainty of diagnosis on the part of the neurologists, combined to prevent operative interference in such cases, patient after patient perishing without even an attempt at treatment.

In June, 1887, Mr. Victor Horsley operated upon a case, the symptoms of which had begun immediately after a severe traumatism in 1884, and were thought to indicate an intercostal neuralgia, localized beneath the lower part of the left scapula. For a long while the pain was the only marked symptom, but in February and March, 1887, there came on distinct loss of power in both legs, and by June the symptoms were those characteristic of a transverse lesion of the cord a little above the middle of the dorsal region.

Mr. Horsley operated very much after the method employed many years previously by Gordon, McDonnell, Hutchison, and others, trephining the laminæ of the fifth vertebra after having first cut off the fourth, fifth, and sixth dorsal spines. After the removal of the trephine button, the rest of the laminæ of the fourth, fifth, and sixth vertebræ were then taken out by means of the bone-forceps and knife. The dura was opened, but nothing abnormal was discovered. The third and seventh laminæ were then removed, and finally the lamina of the second dorsal vertebra, disclosing an oval or almond-shaped tumor, which subsequently proved to be a fibro-myxoma. This was easily removed, when the lateral column on that side was found to be so notched by pressure that the bottom of the groove nearly reached to the middle of the cord. The patient made an excellent and fairly rapid recovery, and one year later was able to do a day's work of sixteen hours, including much standing and walking about.

In October, 1888, I operated on a case of paraplegia thought to be due to focal spinal lesion, possibly to a tumor, in a patient of Dr. F. X. Dercum's. The case will be found reported very fully in the *Annals of Surgery* for June, 1888. Although no tumor was found, the removal of peri-dural connective tissue which had undergone some thickening, and the separation of subdural adhesions were followed by very marked improvement in sensation and mobility, which improvement has gone on to entire recovery.

On December 25, 1887, he was attacked with severe pains in the arms and shoulders, burning and shooting in character and attended by pain on motion, but no loss of power. He described it as rheumatism.

Three or four days later, however, he noticed distinct weakness of the thighs. This weakness increased and spread rapidly down the legs to the feet, and upward over the trunk as far as the breast, and, in the course of eight days, had deepened into absolute paralysis of the parts involved, including both sphincters. At the same time the paralyzed parts became the seat of profound anæsthesia. The latter extended up to the level of the nipples, at which point an intense girdle-pain made its appearance. A bed-sore, trophic in character, followed in time, and added to the patient's sufferings. All the reflexes of the legs, both deep and superficial, became exaggerated. Percussion of the spine elicited pain over the third and fourth vertebræ and to a less extent over the fifth. Flexion and torsion of the trunk also gave rise to pain in this region, while slight blows upon the head in the direction of the spinal axis gave rise to frightful exacerbations of the girdle-pain.

For upwards of ten months this man's condition had grown gradually worse, until the full development of his symptoms, as detailed, had been reached. His general health had suffered severely, and certainly the prospect of recovery was anything but hopeful. Internal therapeutics had been exhausted, and, after a consultation of the neurological staff, it was decided to trephine the spine. The operation was regarded as exploratory in character, though the symptoms pointed directly to a localized affection of the spine or of its immediate contents.

The fifth, fourth, third, second, and first dorsal spines and laminæ were successively removed, and the exposed dura, which seemed unusually thickened and resistant, was incised and opened for a distance corresponding to the length of the wound. The dura was found to be very adherent to the subjacent pia mater by numerous fine bands of new connective tissue, especially in the upper angle of the wound. These adhesions were separated with some little difficulty. The cord was then cautiously explored with the finger, but nothing further was found. The subsequent surgical progress of the case was all that could be desired.

A few hours after the operation, the effect of the anæsthetic having entirely passed off, the patient stated that the girdle-pain had entirely disappeared; nor did it at any subsequent time return. On the following day there was distinct though very slight return of sensation in the feet, and this steadily increased day by day. Errors of location as regarded the level of the impression, and allocheiria, were present and quite persistent. On the sixth day a barely discernible voluntary movement of the toes of the right foot was observed. Little by little a return of voluntary motion obtained, until some two months later, when he could distinctly move the feet and the muscles of the thighs. Sensation, too, steadily improved, allocheiria disappearing, a few errors of location being made. In addition the bed-sore had promptly healed, and some slight control had been regained over the sphincters. This was the patient's condition up to April, 1889. The operation had been performed October 17, 1888.

During the months of May and June the gradual and gratifying return of spinal functions continued, and by July very good control of the urinary sphincter had been regained. He was now capable also of extending and flexing the legs and thighs at will, and a little later he insisted on being raised to the sitting posture. About this time also his control over the bowel became more decided. His improvement continued unabated until the latter part of August, when he had the temerity to

attempt a few steps out of bed. He was, however, restrained in his efforts in this direction for some time longer, and his ambition limited to sitting up in a rolling-chair. In getting in and out of bed, however, he showed such a decided return of power that limited efforts at walking were soon permitted. His progress now was relatively rapid, and he soon learned to walk the entire length of the ward with the aid of a cane. At present he has completely recovered, and is now out of the hospital, earning his living by manual labor.

The technique of Horsley as to his successful case of spinal tumor has been followed in three other cases, once with success by Pescarole, who removed a subdural tumor compressing the cord from the second to the fifth dorsal vertebræ. In the other two cases, one, also of Horsley's, died of shock. Sonnenburg operated for a rapidly-developing sarcoma, which could not be completely removed. Death occurred in six weeks. Five other cases of trephining for tumor are as follows:*

Reydellet ("Dictionnaire des Sciences Médicales," vol. xxxiii. p. 564, 1819) exposed the cord by accident during the removal of a hydatid cyst. The patient died a year later, as a result of suppuration developed in the cyst. Athol Johnson (*Trans. Path. Soc.*, London, 1856–57, vol. viii. p. 16) removed from a child of ten months a congenital tumor, which proved to be a lipoma. It was at the level of the first sacral vertebra. A portion of the tumor was found later to be within the dura. This was not touched. The patient died in six weeks, of peritonitis. Wright (Thorburn, *op. cit.*) removed a portion of a fibro-sarcoma of the neck involving the brachial plexus and invading the spinal canal. The tumor lay external to the canal, but appeared to communicate with it by a diverticulum penetrating the third intervertebral foramen. The tumor was removed, but the operator merely curetted this tract. There was some improvement. Bardeleben (*Berlin. Klin Woch.*, Nov. 4 and 18, 1889, Nos. 44 and 46, pp. 946 and 1004) removed a tumor adherent to the seventh and eighth ribs and to the vertebral arches, between which it penetrated. This portion appears to have been left undisturbed. Death occurred in a few hours, and at the autopsy a nodule was found lying upon the posterior portion of the dura mater and compressing the cord. Abbé has reported (*N. Y. Med. Record*, 1890, vol. xxxviii. pp. 85–92) a case of pressure paralysis from extra-dural sarcoma. The operation was performed after the symptoms had existed for eight months. He resected the arches of the eighth, ninth, and tenth dorsal vertebræ and completely removed the tumor. The patient died on the ninth day.

A sixth case, reported in the *Neurolog. Centralblatt*, by Laquer, was operated on by Rehan on account of loss of sensation in the lower extremities, abolition of the patellæ reflexes, continuous and severe sacral pain, etc. A small extra-dural lymphangeioma was found, compressing the cauda equina. It was easily removed. Two weeks later all pain had disappeared, and four months afterwards recovery was almost complete (*Med. and Surg. Reporter*, September 12, 1891).

Five cases of intraspinal section of the posterior roots of the spinal nerves for obstinate neuralgia have been reported. 1. Those of Abbé are summarized as follows: *a.* Intractable brachial neuralgia, nerve-stretching, amputation, and finally intra-dural division of the sixth, seventh, and eighth cervical nerves; no permanent recovery. *b.* Intractable brachial neuralgia; intradural division of the posterior roots of the sixth, seventh, and eighth cervical and first dorsal nerves; recovery; improvement (*Medical Record*, 1890, vol. xxxviii. 2, pp. 85–92). 2. Bennett reported (*Lancet*, April 27, 1889, i. p. 839) a case in which he had divided close to the cord the posterior roots of the first, third, fourth, and fifth lumbar pairs and first and second sacral nerves. The spasms from which the patient suffered continued, but the violent neuralgic pains disappeared. The man died in fifteen days, of cerebral hemorrhage. 3. Horsley has reported two analogous cases in the tract of the eighth and ninth dorsal nerves. The posterior roots of these two nerves were divided. There was slight improvement, but later involvement of other nerves. The second case was a boy 15 years of age. He suffered from violent pains, vaso-motor troubles, and atrophy in the region supplied by the nerves coming from the left path of the lower third of the cervical enlargement of the cord. The seventh and eighth posterior roots were divided on the left side. Cerebro-spinal fluid escaped for three weeks. There was considerable improvement. Chipault suggests that such operations as the foregoing can only be applicable to cases of exceptional gravity. In such cases their advantages are,—1, to limit with certainty the route of an ascending neuritis; 2, in cases of difficult diagnosis to permit of the exploration of the vertebral canal; and, 3, to limit the section to the posterior nerves, thus avoiding paralysis of the territory innervated by the anterior nerves.

We may now consider the possible indications for operation in this third group of cases, or those in which there is reasonable ground for suspecting the existence of a removable neoplasm. Naturally, this group will, more than any other, try the diagnostic powers of both surgeon and neurologist, and no broad view of the subject can be taken without a brief enumeration of the various sources of error. This will lead me upon ground properly belonging to the neurologist, but I may perhaps be pardoned if I give a brief outline of the general diagnostic points which it will be necessary to bear in mind before deciding upon or rejecting operative interference. In the first place, it will be requisite to determine whether the symptoms are due to pressure on the cord or to inflammatory or other changes involving primarily the structure of the cord itself. If, in a case of paraplegia, we have the history of a very gradual onset, beginning with pain and followed first by motor paralysis and then by sensory paralysis; the symptoms being irregularly unilateral; the pain having first seemed to be neuralgic or rheumatic and burning and shooting in character; if the pain and anæsthesia in the lower limbs ascend gradually from the soles of the feet towards the trunk; and if at the same time there is a constant dull ache in a distinct segment of the spinal column, accompanied by a feeling of weakness at that point, much heightened by

*Chipault, *op. cit.*

FIG. 2.—POSTERIOR VIEW OF PATIENT, SHOWING CICATRIX. ("Annals of Surgery.")

FIG. 3.—LATERAL VIEW OF PATIENT. SHOWING ATTITUDE AND GAIT. ("Annals of Surgery.")

fatigue, we have a group of symptoms pointing strongly in the direction of a neoplasm. In addition, it would be found that at first the reflexes, both deep and superficial, were much exaggerated, becoming gradually lost as destruction of the cord with descending degeneration and wasting grew more marked, the abolition of the reflexes beginning, as in the case of the pain and anæsthesia, in the plantar region and passing gradually upward.

Later in the disease we have the development of spasms with clonus, most marked in the intra-dural cases. The same remark is true of the symptom of rigidity. Local nutrition is not usually impaired; tenderness of the spine on percussion when it occurs in the dorsal region appears to be lower than the tumor producing it; in the cervical region, according to Horsley, this generalization does not seem to hold so closely. The feelings of stiffness and weakness will usually be found to correspond to the position of the tumor. Lateral curvature of the spine is a secondary result of the tonic spasm of the spinous muscles, and therefore the concavity of the bend is on the same side as the growth. The pupils are not affected, except when the cord is pressed upon above the level of the second dorsal nerve.

The age of the patient, judging from Mr. Horsley's tables, throws but little light upon the diagnosis of tumor, as we find lipomata occurring at an average age of 21½ years, sarcomata at 18 years, echinococcus at 34 years, tubercle at 39 years, scirrhus and myxoma at 48 and 53 years. These figures apply to extra-dural growths. In the case of intra-dural growths we have tubercle at 18½ years, myxoma at 43 years, fibroma at 44 years, sarcoma at 41 years, psammoma at 51 years. It will be seen, therefore, that while age may be of use in eliminating certain forms of growth, as, for example, tubercle in intra-dural growths in persons beyond 30 years, it is of little value in deciding the general question as to the presence or the absence of a neoplasm.

This sketch of the principal symptoms makes a tolerably distinct clinical picture, but one which is, nevertheless, liable to great variation, and therefore difficult to differentiate from that of certain conditions of the cord, some of which are due to causes quite beyond the reach of operation.

The chief of these, with the most obvious diagnostic points, are as follows:

Spinal Hemorrhage: Intra-medullary.—Onset sudden; history of traumatism, or of disease associated with profound blood-changes; symptoms bilateral; pain in back; rapid disappearance of reflexes connected with the affected segment; spasms and rigidity and paralysis appear rapidly; girdle symptom; bed-sores; incontinence of fæces; retention of urine very early in case; rapid course; often fatal.

Extra-medullary.—Pains in limbs, spasms, etc., more marked; paralysis supervenes more slowly; course of case much slower and much more favorable.*

Pachymeningitis Externa, from Caries.—Common cause, caries of vertebræ; family history of phthisis, scrofula, or tubercle; personal history fair; patient weak

* "The diagnostic symptoms of meningeal hemorrhage, as given by Gowers and others, and according to my own experience, are as follows: 1, sudden and violent pain in the back, less or more diffused; 2, pain along the course of the nerves passing through the membrane near the extravasation; 3, abnormal sensations,—tingling, etc., and hyperæsthesia,—referred to the same parts; 4, spasm involving vertebral and other muscles supplied by affected nerves, and also sometimes the muscles supplied by the cord below the seat of the hemorrhage; 5, sometimes general convulsive movements; 6, sometimes spasmodic retention of the urine; 7, consecutive paralytic symptoms, but not usually complete paralysis.

"Some points of differential diagnosis between meningeal hemorrhage and extravasation into the substance of the cord should be borne in mind. Symptoms of irritation, such as pain, hyperæsthesia, paræsthesia, and spasm, in meningeal hemorrhage, are usually immediate or very early, and may precede paralysis, which is commonly not complete. In hemorrhage into the substance of the cord, paralysis may be very complete at first, or rapidly become so, and symptoms of irritation may be very largely wanting. Hemorrhage may, and not infrequently does, involve not only the membranes but also the substances of the cord, giving complex symptoms" (Chas. K. Mills, "Spinal Localization," THERAPEUTIC GAZETTE, May 15 and June 15, 1889).

"In some cases of intraspinal hemorrhage, whether primary or secondary, trephining might be performed in two places in order to make sure of a good result,—namely, at the supposed seat of the extravasation, and the lowest portion of the lumbar spine, where, as already indicated, much of the extravasated blood would gravitate. Kronlein, it will be remembered, has recommended that in cases of cranial meningeal hemorrhage two trephinings should be performed if one is not successful, and several cases of failure in trephining for intracranial hemorrhage have been reported which would have been successful had the second operation been performed. In like manner two trephinings of the spine, or a single trephining at the lowest possible place in the lumbar region, might prove successful where failure would otherwise result.

"The somewhat frequent occurrence of spinal hemorrhage with fracture of either body, spines, or laminæ of the vertebra must not be overlooked. The diagnosis of the exact height of the lesion and the decision as to trephining may often have to be settled negatively or positively through a consideration of the question of the probability of a double lesion in cases of accident,—namely, both fracture and hemorrhage. The fracture and the hemorrhage may be situated at different levels, or, if the hemorrhage occurs primarily at the seat of the fracture, it may find its way down the canal" (Chas. K. Mills, "Spinal Localization," THERAPEUTIC GAZETTE, May 15 and June 15, 1889).

and feeble; usually history of injury also; onset slow; first symptoms often rigidity of spine, with tenderness on pressure or on tapping top of cranium, or on flexion or rotation; muscular palpitation and weakness rather than spasms or paralysis; irregular hectic fever; characteristic deformity; spinal abscess; bladder and rectum rarely affected; emaciation.

Chronic Transverse Myelitis.—Frequently a history of acute myelitis; slow in onset; bilateral; first symptom apt to be an impairment of motion, ranging from paresis to absolute paralysis; increased reflexes; ankle-clonus; rigidity common, but paresis, contractions, etc., more marked than paralysis; pain in back and girdle symptom common; anæsthesia and paræsthesia proportionate to motor paralysis; bladder and rectum not greatly involved; slow course.

Primary Lateral Sclerosis, or Spastic Paraplegia.—Often attacks robust adults between 30 and 50; slow in its onset; bilateral; begins with a feeling of weight and weakness in lower limbs, preceded by a peculiar and characteristic gait; spasm chiefly of muscles of lower extremities; manipulation of those limbs throws them into tonic spasm; paralysis not very common early; paresis and rigidity are characteristic symptoms, together with arched spine during peculiar locomotion.

Hypertrophic Cervical Meningitis.—Symptoms affect chiefly the upper limbs, with upper part of spine and thorax; bilateral; marked pain and rigidity, involving all these regions; paralysis occurs in second stage, and affects first and chiefly arms, shoulders, etc.; no atrophy of lower extremities; no bed-sores; no bladder or rectal trouble.

Having decided in any given case that the symptoms are probably due to tumor, the interesting question whether it is intra-medullary or extra-medullary will still remain to be settled. The best general guide will be found in the fact that the symptoms of intra-medullary growths are chiefly those of motor and sensory impairment, while the extra-medullary growths produce much more markedly irritative symptoms, as, for example, pain, spasms, etc. In the presence of a paralysis of gradual development, preceded by long-continued signs of nerve-irritation, and with a distinct unilateral element, the transference of paralysis from one to the other limb having been effected slowly and after a considerable interval, the diagnosis of compression of the cord by some cause outside of its own structure would seem warranted. Aneurism might be excluded in the absence of the characteristic physical signs and of evidence of erosion of the spinal column; gumma would be accompanied by a history of syphilis, would often be associated with other and recognizable specific lesions, and would possibly yield to the use of iodide of potassium; a growth (cancer or tubercle) of the bodies of the vertebræ themselves usually causes a perceptible deformity. By attention to the points which I have mentioned, a reasonably correct opinion may be arrived at, but I would strongly advise that in no case should the surgeon undertake a spinal resection based on a diagnosis of tumor without having called to his aid a skilled neurologist and carefully studied the case with him. The same remark applies with even more force to the question of localization. The broad general principles long applied in cases of traumatism are of course equally applicable to tumors, but there are numerous refinements of modern diagnosis which belong especially to the province of the neurologist and which are of vital importance in selecting the site of operation in cases of supposed tumor. Horsley's experience in the patient of Dr. Gowers is ample evidence of the unavoidable uncertainty as regards the particular vertebra or vertebræ to be attacked, but there is every prospect that these diagnoses will become more and more scientifically exact in the near future. In the mean while I would refer the surgeon who desires carefully to study for himself this side of his cases to the articles referred to below.*

Having decided that the probability is in favor of the existence of a tumor, and having also fixed upon its probable seat, the surgeon is confronted with the important question of the advisability of operation; in other words, with the question of prognosis.

All the evidence which we now have points to extraordinary reparative power on the part of a cord which has simply been suffering from compression, and to an almost equally remarkable tolerance of operative interference. The material for finely-differentiated prognoses can hardly yet be said to exist, but in a general way it is safe to say that the diagnosis of tumor (if it be non-malignant) carries with it a reasonably favorable prognosis, which is strengthened if, in addition, the tumor is thought to be extra-medullary.

As a result of the above considerations it seems to me proper to conclude that every case of focal spinal lesion thought to depend

* Seguin, "Localization of Spinal Lesions" ("Pepper's System of Medicine," vol. v. p. 77); Starr, "Localization of Functions of the Spinal Cord" (*American Journal of Neurology and Psychology*, vol. iii., 1889); Gowers, "Diseases of the Nervous System," vol. i. p. 142; Thorburn, *The Medical Chronicle*, April, 1889, p. 1, and the *British Medical Journal*, December 22, 1888; "A Contribution to the Surgery of the Spinal Cord," 1890; Ranney, "The Applied Anatomy of the Nervous System," p. 355; Ross, "Diseases of the Nervous System," p. 356; Charcot, "Localization of Cerebral and Spinal Disease;" Bramwell, "Diseases of the Spinal Cord;" Mills and Lloyd, "Pepper's System of Medicine," vol. v.; Mills, "Spinal Localization" (THERAPEUTIC GAZETTE, May 15 and June 15, 1889).

on a tumor and not distinctly a malignant and generalized disease should be regarded as amenable to operative interference, no matter how marked or how long continued the symptoms of pressure may be.

D. The last group of cases, those of spinal traumatism, is extremely interesting when viewed from the operative stand-point.*

Operation in cases of injury was suggested as far back as the days of Paulus Ægineta, who taught that in spinal fracture the surgeon must, if possible, attempt to extract the compressing bone by incisions, having first warned the patient of his danger (Adam's translation). Ambrose Paré in the presence of paralysis made a like recommendation. Heister advised removal of any fragment that pressed on the cord. In addition, Hildanus, Matz, and Vigaroux all referred to the operation, though none of them preferred it, while Heine, Roux, Holscher, and Dupuytren each removed portions of the vertebræ for disease. Malgaigne says Chopart and Desault are the first to speak of it as a very simple thing "to trepan between the spinous and transverse processes, to give issue to effused fluids or to elevate or remove pieces of bone which may wound the cord;" but it is not recorded that their suggestions bore any fruit, and a long interval elapsed without further mention of the subject. Dr. James, in his "Medicinal Dictionary," London, 1745, said (in almost the same language employed by Heister),—

"If the spinal marrow is wounded, death follows inevitably. Though as it may seem cruel not to attempt the relief of one under these unhappy circumstances, the surgeon should lay the injured part bare by the knife, and elevate the fragments, which press upon the medulla, in a proper manner; then let him cleanse the wound thoroughly and apply balsamic medicines, using the napkin and scapulary. He must continue this until the wound heals or the patient dies."

The nearest approach to a formal resection of the spine at about this date was the operation of Louis in 1762, reported in the *Arch. Générales de Méd.*, 1836, vol. ii. p. 417, and performed upon M. de Villedon, captain in the regiment of Vaubecourt, who was paralyzed from a gunshot wound of the back, received at the battle of Aménébourg. Louis and Duplessis consulted on the third or fourth day. They withdrew fragments of bone. They also spoke of "the indication for the trephine, even without fracture, to give exit to blood or matter, or even to prevent suppuration in some cases of fracture." M. Villedon recovered and was finally able to walk. Louis concludes, "Quoi qu'il en soit, c'est une victime que l'art a soustrait à une mort certaine."

The formal operation for the removal of a depressed vertebral arch was, however, first performed by the younger Cline, in the spring of 1814, and, though unsuccessful, showed the practicability of such an operation; Cline considered (South's Notes to Chelius's Surgery) that the symptoms sometimes resulted from the pressure of the spinal cord by the spinous process or arch of the displaced vertebra, either fractured or not, a condition which he thought analogous to that of the brain when a piece of the skull is driven in upon it and causes symptoms of compression, which are relieved by the removal of the cause of the compression. With this notion he held it feasible to remove the compressing vertebral arch, and so to relieve the palsy. In the clinical lecture which he delivered he explained the reasons which had induced him to perform this operation, and the grounds upon which he hoped for success.

He considered that in fracture with displacement of the vertebræ, which compressed the spinal cord, this great nervous cord was under precisely the same circumstances as the brain when pressed by fractured skull, and that, therefore, as the elevation of the pressing bone was indicated in the latter case, and often effected with success, so was it equally called for in the former, and that no positive reason could exist why the operation should not be successful, provided the injury of the spinal cord itself were not great, a condition which, as regarded the brain, could equally forbid operation, or render its success improbable or impossible. Before undertaking the operation of cutting out that part of the arch of the vertebra which compressed the cord, Cline proposed to himself the following four questions, to which he gave the annexed replies: (1) Will the patient die of the operation? Probably he will, if the injury be severe.† (2) If the cord be much hurt, will it recover its functions? This is unknown; but we do know that if a nerve be divided it will unite, and the greater part of the spinal cord may be divided in a brute and yet the animal recover; in proof of which he detailed an experiment which he performed on a bitch. (3) After the removal of the arch of the vertebra will the spine be sufficiently strong to support the body? Probably it will for ordinary purposes, though the patient may not be able to lift heavy weights. (4) Will a patient recover from a compound fracture of the spine, which, by the performance of the operation, it becomes? The nearer a fracture is to the source of the circulation, and the less compact, and consequently the more vascular, the bone is, the greater is the probability of recovery. Both these advantages are present in the spine, and therefore favor the successful issue of the case. He added that the only reasonable objection to the operation of trephining the spine is, that we cannot, previous to the operation, ascertain whether the spinal cord is simply compressed, or whether it is partially or entirely torn through, or whether the symptoms of compression result from the effusion of blood in different situations, neither of which indeed can be ascertained after the vertebral canal has been opened, unless the sheath be rent.

Cline's operation, although his patient died, attracted great attention, and for a time its propriety was vigorously discussed. An earnest argument, often degenerating into bitter and virulent personality, was kept up for many years, Sir Astley Cooper, Benj. Bell, Tyrrell, South, and others endorsing and upholding the operation, while Sir Charles Bell, John Bell, Sir Benj. Brodie, and others violently opposed it. The following quotations give an idea of the tone of a portion of the papers which were published about that period. Brodie wrote of the operation that "even under the most auspicious

* Here and elsewhere throughout this paper I have used freely the material contained in a former paper of my own on the same subject, adding largely to it and bringing the history down to date. (See "Annals of Surgery," June, 1888.)

† South says, "I do not exactly understand the answer given, for there is no danger in the operation, if it be performed with care; but I suspect he thought the exposure of the spinal cord might hasten inflammation, and so death might more quickly result."

circumstances it must be doubtful whether it may not be productive of harm rather than of good to the patient;" while Sir Astley Cooper concludes his remarks on the subject as follows: "Though I may not live long enough to see the operation frequently performed, I have no doubt that it will occasionally be performed with success. There is no reason why it should not; and he who says that it ought not to be attempted is a blockhead."

After a time, however, the controversy languished, although here and there a solitary operation, almost invariably entirely unsuccessful, was recorded.

Dr. Alban G. Smith reported a case in which portions of the third dorsal vertebra were removed for relief of paralysis from fracture. At the time of the report, bed-sores, from which the patient had previously suffered, had healed, and there was return of sensation (*North American Medical and Surgical Journal*, July, 1829, vol. viii. pp. 94–97).

In the *British and Foreign Medical Review* for 1838, in a critique of Sir Charles Bell's "Institutes of Surgery," the reviewer quotes a successful case, in which the operation was performed by a Welsh surgeon by the name of Edwards, resection having been made in the lumbar region. The case, however, is very imperfectly reported. The cases of Tyrrell, Barton, Wickham, Smith, Rogers, and others were all unfavorable, and Malgaigne, after having reviewed their results, joined the ranks of the opponents, deciding that the fatality of the recorded cases should be sufficient to deter surgeons from so grave an operation. He believed that if the ordinary methods were not sufficient in a case of fracture with displacement, we might try to act on the posterior fragment with forceps or tenacula to draw it backward, but that "the extraction or trephining of the arch presents itself as so desperate a resource" that he would not advise any one to adopt it.

Mayer reports a case of paraplegia following injury in which he removed a portion of the sixth and seventh dorsal vertebræ with some temporary improvement, but followed by death on the twenty-first day with all the symptoms of sepsis (*Journal der Chirurgie und Augenheilkunde*, Berlin, 1848, vol. xxxviii. pp. 162–200).

In 1861, Dr. Hutchison, of Brooklyn, published (*American Medical Times*) the history of an operation for fracture, which resulted in death, and reviewed the histories of the recorded cases up to that time. He did not arrive at any general conclusion as to the propriety of surgical interference.

Dr. A. A. Potter reports three cases of operation for paralysis following fracture. In one of them no improvement followed. In another death resulted in four days. In the third, in which there had been complete paralysis of motion and sensation, a perfect recovery was effected (*American Medical Times*, 1863, vol. vi. pp. 17, 18).

Laugier (Felizet, *Archiv. Gèn. de Mèd*, October, November, and December, 1865), on the fourth day after a fracture of the spine, removed the eighth dorsal spinous process, which was broken, and trephined the arch of the ninth dorsal vertebra. The patient died on the fifth day after the operation. Fracture of the body of the eighth dorsal and comminuted fracture of the body of the ninth dorsal vertebra were found.

In 1865 considerable attention was again drawn to the operation by Dr. Robert McDonnell, who reported the case of Dr. Samuel Gordon, in which trephining of the twelfth dorsal vertebra had been followed by the healing of a bed-sore, the disappearance of a virulent cystitis, and the return of power over the bladder and of sensation in the rectum, the patient being still alive, but unable to stand, at the end of five months.

Dr. McDonnell also reported a fatal case of his own, and reviewed twenty-six cases of trephining, in seven of which life was preserved for a long time. He called attention to cases of temporary paralysis from pressure by pus, in which the cord had entirely recovered, and to similar ones of paralysis from wounds of the spine, followed by recovery on the withdrawal of the foreign body which was causing the compression.

He concluded that instances of fractures of portions of the vertebræ, unaccompanied by fracture or displacement of the body, are not very infrequent, being ten per cent. of the recorded cases which he was able to discover, the majority of them occupying the cervical or upper dorsal region.

He suggested that the loss of reflex movement in such patients might be due to a venous congestion of the cord below the seat of injury, not to structural change, and that such loss is therefore an additional indication for operation.

He thought that evidences of myelitis or meningitis, the girdle symptom, formication, cramps, spasms, fever, etc., contraindicated operation; but believed that the softening of the cord under pressure was quite different from ordinary inflammatory softening or "white softening," and that it indicated no structural change from which the cord could not recover.

In the discussion in the Royal Medical and Chirurgical Society (*Medical Times and Gazette*, December 9, 1865, vol. ii. pp. 639, 640) which followed the reading of Dr. Gordon's paper, Mr. Alexander Shaw called attention to the fact that in this successful case the operation was not performed until the second month after the accident, during which time the vertebral column must have undergone essential changes; so that, if the bones had not perfectly united, at least all the adjacent parts must have become consolidated by the processes of repair.

"And," he added, "observations show that when patients have passed through the first stages of the accident, and have lived long enough for the injured parts to acquire some solidity and firmness, their chances of surviving, although with paraplegia remaining, are greatly increased; the probable reason being that, as they can be moved more freely in bed, and their positions more frequently changed, the bed-sores are more likely to be cured."

In the same year Felizet (*op. cit.*) reviewed twenty-six cases, and reached the following conclusions: 1. The cause of death after fracture of the vertebræ is the compression or continued irritation of the spinal cord. 2. The operation should not be held responsible for the subsequent symptoms, as they are equally grave in cases where no interference has been practised. 3. Trephining, on the contrary, conforms to physiological conditions, causes almost immediate improvement if the cord were not completely destroyed, and in those cases where it fails, the failure is almost always due to the gravity of the traumatism itself.

In 1866, Mr. Jonathan Hutchinson ("Clinical Lectures and Reports of the London Hospital," vol. iii. p. 327) considered at length the conditions usually met with in spinal fractures and luxations, with the object of furnishing conclusive arguments in support of the usual practice at the London Hospital, of abstaining from operative interference in such cases.

He asserted that a great many cases recover, if put under favorable circumstances and let alone, and that of those which end in death, very rarely indeed could it be said, after post-mortem examination, that an operation could by the barest possibility have done any good. In the great majority, then, since it could have done no good, its effect would have been to increase the patient's sufferings and aggravate his danger.

With regard to the lesions usually met with in the post-mortem room, he made the following assertions, based on the examination of about twenty cases: "1. Permanent compression of the cord, or of any part of it, is a very rare event. From my experience I should not think that permanent displacement to any material degree is met with once in ten cases. 2. As might be expected from the fact that almost all the more serious injuries to the spinal column are due to indirect violence (bends), the fractures of the laminæ are of little consequence, and never cause compression of the cord. In almost all our cases the laminæ, spinous processes, etc., are more or less fractured, but I have never yet seen a case in which any of the fragments were in contact with the cord. 3. Having thus denied that the cord is permanently compressed, either by the bodies or the laminæ, I now extend my statement to extravasations of blood. I have never myself seen any effusion to the extent of possible compression, and in the majority of cases there is little or none. The injury is a crush, and is not one at all likely to cause much bleeding. There are no large arteries to be injured. 4. Instances of great displacement of one vertebra do sometimes occur. They are exceptional, however, and very rarely of a kind which we could rectify by force or by operation. 5. The cases in which during life there is evidence of considerable displacement are not by any means always the most serious ones. The irregularity, perceived externally, concerns rather the spinous and transverse processes than the bodies, and it is very possible that they may have been fractured without any crush of the cord; and, on the other hand, that the bodies may have been displaced for a moment, allowing complete crushing of the cord, and yet no permanent irregularity may have resulted. In justification of the strong opinion I have already expressed against operations with the intention to elevate depressed portions of bone, I must say a few words more. *My chief reason is that by doing so you convert a simple into a compound fracture, and add the risks of pyæmia, together with those of spinal meningitis.* Then I urge that depressions of bone very rarely exist, perhaps not one in twenty cases, and that it is utterly impossible to select the case."

Mr. Maunder reports the case of a man aged 28 who, having paraplegia as a result of an injury, was operated upon nineteen days later.

The spines of the first and second dorsal vertebræ were cut off, and the corresponding laminæ removed by the trephine and bone-forceps. The patient died of pyæmia on the thirteenth day. At the autopsy, the cord was found to be "pulped" opposite the lower border of the seventh cervical vertebra (*London Lancet*, 1867, i. pp. 237, 238).

In 1867, Dr. John Ashhurst reviewed the subject up to that date, publishing a table of twenty-six cases, many of which were included in Dr. Hutchison's and Dr. McDonnell's collections, reviewed their clinical history, said that there was not in fact one well-authenticated instance of recovery from spinal resection, and concluded that in no case did resection or trephining offer a reasonable prospect of improving the patient's condition; but that, on the contrary, there was reason to fear that they increased the chances of fatal termination.

In 1869, Mr. Thomas Nunneley, in an address on "Surgery" before the British Medical Association, again brought the subject to the attention of the profession. He said that while the great majority of surgeons had had an unfavorable opinion of the operation of resection of the spine after fracture or dislocation with symptoms of compression, and while for many years it seemed to have been almost forgotten, yet he could not but think that this position had resulted more from submission to habit or to traditional authority than from a due consideration of the subject. He reported four cases of his own with one recovery, reviewed the tables of McDonnell, and concluded that out of thirty-three cases of fractures in various parts of the spine, life had been permanently saved in three and considerably prolonged in three others, and that in still others general improvement had resulted. He called attention to the fact that all the successful cases were most unfavorable at the time of the operation, and should, therefore, not be compared with large collections of injuries to the spine, of all grades of severity. His general conclusion, therefore, was distinctly favorable to the operation, and he quoted with approval the dictum of Sir Astley Cooper, who, more than once, declared that if one patient out of one hundred could be saved it would be better than could be hoped for from nature alone. During the same year, however (1869), Mr. LeGros Clark, in a lecture on "Diseases of the Spinal Cord" (*British Medical Journal*, July 17, vol. ii. pp. 49-52), took equally strong ground against the operation. His argument may be summarized as follows:

The end proposed in an operation of this kind is to remove displaced bone which is supposed to press upon or irritate the cord. But such a proceeding is most likely to prove abortive from the inaccessibility of the displaced bone, fractures of the arches alone being vary rare except from direct violence. But the direct violence which would suffice to fracture the arch and drive the spinous process into the cord, the body of the vertebra remaining unbroken, must almost inevitably prove hopelessly destructive to the nerve-tissue. Again, if the surgeon operate early, he does so before it is known whether the injury is permanent, a fact which can only be developed by time; if he wait he diminishes his chances of success. If he does operate, he converts a simple fracture into a compound fracture, and a communication is established with a canal having the most delicate and susceptible relations, and which must be liable to the intrusion of the products of inflammation during such period as the patient may survive the effort to repair the lesion. If the cord has been crushed, and the operation has been consequently useless, probably life may be thereby only curtailed; but if (as must be presupposed for the operation to have a chance of success) the cord be not crushed, it appears that the best chance for the patient's recovery is merely extinguished. The operation has been advocated on the *erroneous hypothesis that the spinal cord can be compressed without serious disintegration of its texture. I believe this is scarcely ever the case*, unless such pressure result from extravasation of blood, a condition which, if it could be ascertained, would not be regarded as indicating operation.

The only supposable form of spinal surgery which, in

my opinion, might be benefited by operation is a fracture of the vertebral arch alone, with limited depression, or the recent intrusion of a spiculum of bone within the theca, whereby the cord is pricked and irritated. But where are these cases to be met with, and how are they to be recognized? I fear we must abandon this operation. It can scarcely be alleged by its advocates that, if unproductive of good, it is harmless. To weaken still further the remaining connections of a broken spine, to convert a simple into a compound fracture, to expose the sheath of the cord, and possibly the cord itself, and to entail the risks attending the period of repair, cannot be regarded as circumstances of indifference.

At present, with every disposition to regard this subject impartially, and to give their due weight to the arguments and facts which have been advanced in its favor, I cannot regard trephining the spine as brought within the pale of the justifiable operations in surgery.

In 1870, M. Brown-Séquard, from the point of view of a neurologist, as well as of an experimental physiologist, upheld very strongly the theory that death after fracture of the spine is usually due to the effect of continued irritation of the cord by pieces of broken bone, and not to the results of a partial or complete section of this nervous centre ("Physiology and Pathology of the Nervous Centres").

He quoted the clinical history of many cases to prove that section, or even crushing of the spinal cord, had not proved fatal, and emphasized the fact that in animals death is rarely caused by complete section of the cord in the dorsal region, while they die as quickly and as often as men after fracture of the spine if the broken pieces are not removed. He further called attention to the removal of the body of the third cervical vertebra by an Italian surgeon in a case of syphilitic caries, to another analogous case also operated upon by an Italian surgeon, and to two cases of cyst in the spinal canal with erosion and expulsion of portions of the vertebræ, all of which recovered.

He believed that, as the result of his investigations, the following conclusions were justifiable: 1, that the laying bare of the spinal cord was not a dangerous operation; 2, that death after a fracture of the spine is usually due to the effects of pressure, or of excitation of the spinal cord, and is not the result of a partial or a complete section of this organ; 3, that reunion may take place after a wound of the spinal cord, so that its lost function may return; 4, that the removal of some parts of the vertebræ may be followed by a production of new bone; 5, that the cases of fracture of the spine in which the trephine has been applied show the usefulness of this operation.

In spite of this pronunciamiento, another long interval ensued, with very few reported cases which I have been able to discover of resection of the spine in case of injury. Two cases in 1870, operated on by Cheever, of Boston, both resulted fatally, as did one operated on by E. R. Willard in 1871, and one by Lucke in 1880. Stemen, of Indiana, in 1883, reported three cases,—one relieved, one not benefited, one fatal in eight days.

In 1884, Dr. John A. Lidell, in the article on "Injuries of the Back," in volume iv. of the "International Encyclopædia of Surgery," quoted Ashhurst's statistics, recorded the statement of Professor Eve, of Nashville, to the effect that the operation of resection of the dorsal vertebræ is "one of the most difficult in surgery, if not impracticable," and concluded that the operation of resection or trephining the vertebræ is unjustifiable, because it does not offer a reasonable prospect of improving the patient's condition in any case, while, on the other hand, there is always reason to fear that it may increase the chances of a fatal termination.

In September, 1884, Dr. Halstead, of New York, operated at the Roosevelt Hospital on a patient who, three days previously, had suffered a fracture of the spine of the twelfth dorsal vertebra, with rotation and forward luxation of the body of the same vertebra and fracture of the right transverse process of the first lumbar. The displacement was reduced with the help of Langenbeck's hook, but the patient died seventeen hours later (*Medical News*, January 3, 1885).

In November, 1884, Dr. Pinkerton, acting as house-surgeon for Dr. E. L. Keyes, removed the spinous process and lamina of the twelfth dorsal vertebra, which had been fractured four days previously. The patient died five days later (*ibid.*).

In February, 1885, Mr. William Macewen (*British Medical Journal*, August 11, 1888) operated in the case of a man who, six or eight weeks previously, had had a severe injury to the spine at the level of the lower dorsal vertebræ. He had absolute motor paralysis, incontinence, hyperæsthesia followed by anæsthesia, rapid muscular atrophy, cystitis, bed-sores, pyrexia, etc. Resection was performed. The arch of the twelfth dorsal was found fractured and depressed. A connective-tissue tumor between it and the theca was removed, as well as the laminæ involved. The patient made a good recovery, and a year later could walk about with ease and without support.

In 1886, Dr. Robert T. Morris reported ("Annals of Surgery," June, 1886) the case of a patient who, as the result of a fall upon the head, had complete paraplegia, with loss of sensation and of motion at nearly all points below the neck. Two years and three months after the accident the laminæ and spinous processes of the seventh cervical vertebra and the spinous process of the sixth cervical vertebra were removed. The patient lived ten months, but was not materially improved. At the autopsy very extensive disease of the cord itself in the neighborhood of the injury was found. The membranes were thick and firmly adherent to each other and to the vertebræ.

In 1886, Dr. Carl Lauenstein, of Hamburg, reported (*Centralblatt f. Chirurgie*, No. 51, 1886; *London Med. Record*, March 15, 1887) the case of a patient who, five weeks before coming under observation, had had a fall of forty feet. This was followed by paraplegia, incontinence of urine and fæces, trophic changes, etc. There was a projection in the dorso-lumbar region, most prominent over the spine of the last dorsal. The use of a plaster jacket had failed to give relief. The arches of the last dorsal and first lumbar vertebræ were removed by means of a chisel.

Six months later the patient was able to walk, the incontinence had disappeared, and he was reported as quite recovered.

With the view of deciding the question whether such improvement as had resulted in this case could possibly be attained without the performance of a surgical operation, Lauenstein reviewed the published reports of similar cases, and studied especially the statements referring to paralysis of the bladder. The relief of this condition, he states, is a point of much importance, for, with the exception of bed-sores, which usually occur in such cases, cystitis is, undoubtedly, the most formidable of all the secondary symptoms. Of fifty-three cases of so-called cure of fracture of the spine, collected by Gurlt in his work on fractures, there are fifteen instances of spontaneous restoration of the functions of the bladder and rectum. The shortest interval between the date of the accident and that of complete restoration of the functions of these organs was a few hours, and the longest interval eight weeks.

In not one of six cases of fracture of the spine with paralysis of the bladder, treated by Lauenstein during the last six years, was any spontaneous improvement observed in this latter condition after long intervals. It is concluded from these returns that when, in cases of compression of the cord from fracture of the spine, the functions of the bladder and rectum are restored, this restoration usually takes place after a brief interval, and that if no improvement has taken place in the course of ten weeks, any subsequent spontaneous change for the better in the condition of the bladder can hardly be anticipated. In the case reported by Dr. Lauenstein the bladder symptoms just before the operation were increasing in severity.

Since the introduction of the antiseptic method into practical surgery two other instances in addition to Macewen's and Morris's cases had been reported up to this date (1887) of operative interference for the relief of paralysis from injury to the spine.

The first of these, reported by Maydl, from the hospital practice of Albert, was a case of resection of the ninth and tenth dorsal vertebræ for paralysis following fracture. The dura was opened, but nothing abnormal was seen. The cord was, however, compressed from the upper border of the tenth to that of the ninth vertebra, partly through the fracture of the body and partly through a bilateral luxation between these two vertebræ. The patient recovered from the operation, but was not improved (*Wiener Med. Presse*, 1884, vol. xxv. pp. 1336–1340).

In the second case, reported by Lucke, the operation was performed two days after an injury to the spine, which was found to consist in fracture with crushing of the corresponding part of the cord. This patient also recovered from the operation, but died nine months later from marasmus, and without any but very slight improvement having been observed in the condition of the paralyzed limbs. In six only of thirty-one cases collected by Werner of operations performed on fractured spine in the pre-antiseptic era did the patients recover from the effects of the operation. These results, however, Lauenstein held, cannot be brought forward to discredit the operation of trephining the spine, since sufficient care was not taken in their selection; and the treatment was applied in instances both of recent and of old injury, and even of complete crushing of the cord.

Lauenstein would not operate in a case of recent injury to the spine, as it is well known that occasionally in such instances the symptoms of paralysis disappear spontaneously in the course of a few weeks.

Severeneau reports a case of shot-wound of the back with fracture of the second dorsal vertebra, paralysis of all four extremities and of the bladder and rectum. The ball was recognized with the help of an electrical apparatus and was extracted. Paralysis disappeared, and the patient entirely recovered (*Archiv für Klin. Chirurgie*, 1888, vol. xxxvii. pp. 664, 665).

In the *Edinburgh Medical Journal* for March, 1889, Mr. John Duncan reports three cases of fracture of the spine with paralysis which were operated upon by removal of the laminæ of the broken vertebræ. One case died on the following day; the others recovered from the operation, but were unimproved.

Horsley, in a foot-note to his paper on the case of spinal tumor, says he has operated on a case of fracture, and that the wound healed rapidly, but gives no other details. In the paper itself he also mentions a most interesting case of operation for paralysis following fracture, in which the wound healed rapidly, as did a large bed-sore. There was but little general improvement.

It was found in this case that the spinous process of the eleventh dorsal vertebra was jammed forward between the tenth and twelfth, and also that there was backward compression of the cord against the lamina of the tenth vertebra. These were removed. Mr. Horsley makes the following remarks apropos of these cases, and with reference to the dictum that when there is evidence that the cord has been severely jammed or compressed at the time of accident, an operation is contraindicated:

"In this case no doubt the spinal cord was momentarily jammed at the time of the accident so severely as to practically—*i.e.*, functionally—divide it. Though laminæ were found compressing it at the time of the operation, nevertheless they did not do it so severely as to thereby alone cause the excessive degree of the symptoms, therefore it would seem that the cord, as suggested, must have been compressed at the time of the accident. Now, this is just a case in which, if any attention had been paid to the ruling before quoted, the patient's life would have been lost, and indeed it must be obvious that, considering the necessarily small amount of information on this subject, it should be our duty to operate in every case, since we may possibly do some good, and certainly, if proper antiseptic precautions be taken, we can, to use Mr. Erichsen's words, do no harm.

This question of the damage to the spinal cord not appearing to discount the possible benefit of surgical interference so much as has been hitherto expected, gains additional illustration from the case which forms the subject of this paper, since nothing could well have appeared more hopeless than the indentation of the spinal cord produced by the tumor. This indentation appeared to divide the lateral column completely, and yet, owing doubtless to the gradual character of the compression, the restoration of motor and sensory function has been complete. I would repeat, therefore, that, so far from its being unjustifiable to operate on the spine owing to the possibility of the cord being hopelessly damaged, it seems to me to be criminal not to operate."

Mr. Ernest Hart reports (*British Medical Journal*, March 23, 1889) a case of Péan's in which paraplegia, retention of urine, etc., followed a severe contused wound of the mid-dorsal region caused by the bite of a horse. The spinous processes of the seventh and eighth dorsal vertebræ were not to be felt. Péan removed by operation the fragments of these processes, and of the corresponding laminæ which had pierced the membrane and entered the substance of the cord itself.

He resected the irregular ragged fragments of the transverse processes. Mr. Hart reports that the operation was completed without complication, and that "the patient is now restored to almost his ordinary condition."

Mr. Herbert Allingham reports (*British Medical Journal*, April 13, 1889, p. 838) two cases of fracture of the spine treated by trephining. In one there was fracture with depression of the laminæ of the sixth dorsal vertebra; the spines and laminæ of the fifth, sixth, and seventh were removed. The wound healed in ten days. The symptoms of ascending changes were checked. Improvement to some extent occurred.

In the second case the spines and laminæ of the third, fourth, fifth, and sixth dorsal vertebræ were removed; the wound healed in a fortnight. No mention of the subsequent course of the case is made except the fact that the woman died seven weeks later, and that the cord was found almost divided.

Dr. Robert Dawbarn has reported (*New York Medical Journal*, 1889, vol. xlix. p. 711) a case of paralysis following fracture at the dorso-lumbar junction in which he operated, finding depression of the lamina of the eleventh dorsal vertebra and a fracture of the body of the twelfth dorsal vertebra, which had in part been thrown backward. The depressed lamina was firmly adherent to the theca, and on removing it it was necessary to saw and to elevate the posterior arch of the tenth dorsal vertebra. The cord was found in the shape of a distinct knuckle making an angle of fifteen degrees, its apex pointing backward and lying immediately below the depressed arch. The tenth, eleventh, and twelfth arches and laminæ were removed. The dura was not opened. Ten weeks after the operation all motion and reflexes were still absent. The muscular sense was improved. The power of the bladder was markedly increased. The rectal sphincter was regaining strength. Pain upon motion had largely disappeared. The temperature of the legs had much improved.*

Dr. J. E. Richardson reports (*Brooklyn Medical Journal*, 1889, vol. iii. pp. 401–416) the case of a man, aged 22 years, who had a swelling upon the back of the neck, extending from the third to the seventh cervical vertebra. There was stiffness of the neck, with limited movement of the head and pain on pressure over the cervical spine. After this had lasted for about ten months he began to complain of numbness of the hands, and paresis of the sphincters of the bladder and rectum and of the lower extremity, which finally increased to almost absolute paralysis of both upper and lower limbs. As soon as the patient was anæsthetized, the tumor over the neck instantly disappeared, and it could then be seen that it was caused by the contraction of the muscles of the neck in their endeavor to hold the head and neck erect.

An incision was made extending from the occipital protuberance to near the spine of the seventh cervical vertebra, and, dissecting down to the spinous processes, the lamina of the fifth cervical vertebra was removed with the trephine. The cord was then distinctly seen. It was supposed that there might be an accumulation of pus within the spinal canal, the evacuation of which would remove the pressure. It was found, however, that none existed, and it was decided to do nothing further. The operation was performed antiseptically, and the wound washed out with a solution of bichloride, 1 to 2000; a drainage-tube was inserted into the wound, and the latter then closed and dressed. There was decided improvement in sensation immediately after the operation, but death occurred four days later.

Passed Assistant Surgeon S. T. Armstrong reports ("Reports of the U. S. Marine Hospital Service," 1889, pp. 271, 272) a case of fracture following extreme flexion in which there were complete paralysis, bed-sores, hectic, etc. On October 8 the patient was chloroformed, and resection of the vertebræ was made. An incision was made over the spines of the last dorsal and first lumbar vertebræ, the muscular mass dissected away, and the spines of the twelfth dorsal and first lumbar vertebræ removed, exposing the canal. The membranes of the cord appeared normal, the cord itself being atrophied, and there was no septic fluid in the canal. The anæsthetic was discontinued during the operation, and, as there was anæsthesia of the tissues, the operation was completed with chloroform. There was a "perceptible angling" of the vertebral column, but no pressure was exerted on the cord. The wound was accordingly dressed antiseptically, further operative interference not being indicated.

No improvement in sensation or motion followed the operation. The hectic fever caused by the large granulating and suppurating sores produced by the vasomotor paralysis continued. The patient died October 15. The autopsy showed septic abscesses in various places, and the following condition of the spine itself. On removing the vertebral column a fracture of the body of the third dorsal vertebra was found, almost the entire right half of the body being absorbed. There was also a fracture of the anterior superior edge of the body of the twelfth dorsal vertebra. No fracture of other bones.

In this case the operation was of no benefit, for the pressure on the cord in the region of the twelfth dorsal vertebra was demonstrated as of small consequence. The fracture of the third dorsal was unrecognized during life. At the necropsy no pressure of the cord was found at this site. The large ulcerating surfaces offered an excellent opportunity for septic infection, and it was impossible to heal them on account of the trophic disturbance.

Dr. James Bell reports (*Montreal Med. Journal*, 1889, vol. xix. pp. 911–913) the case of a man, aged 40, who, as the result of a fall, had complete paralysis below the arms and partial paralysis of these extremities. Resection of the laminæ of the fifth and sixth cervical vertebræ was practised, and the dura mater was picked up and incised: a considerable quantity of a reddish cerebro-spinal fluid escaped; the cord looked normal on its surface;

* Dr. Dawbarn thinks that we may now advance the following statements as a rule of practice: "Whenever, following traumatism, even a slight abrupt irregularity of the spinal column is observed to coexist with paraplegia from this level, a cutting operation is indicated to determine whether the paralysis is not, by bony pressure, made incapable of spontaneous relief.

"This operation should be deferred no longer than recovery from the original shock of the injury demands. If needed at all, it is needed early; and we make a mistake if, as in my case, we wait until electricity and time have alike proved futile before attempting what I may call exploratory resection. It will be the easier by far to the surgeon at this early stage, and the safer for the patient; at least when the obvious displacement is due to a broken posterior arch, as then comparatively little bony section would be needed, the fragments not having become consolidated by bony union in their false position."

no evidence of traumatism was to be seen. The patient made a good recovery from the operation, but died suddenly three days later. Post-mortem examination showed a portion of the cord, about three-quarters of an inch long, opposite the sixth cervical vertebra, soft to the touch. No dislocation of the bodies of the vertebræ, and no separation of the posterior ligament.

In another case Dr. Bell resected the lower cervical vertebræ in a case of paraplegia after accident, and found one inch of the cord substance very badly crushed. The operation was therefore terminated. The patient recovered from it, but died comatose three days later from respiratory failure.

Dr. N. P. Dandridge has reported (*Journal of the American Medical Association*, 1889, vol. xiii. pp. 37–43) five cases of fracture of the dorsal spine. In three the plaster jacket was applied, with satisfactory results in two. In one great improvement followed rest in an ordinary bed, increasing markedly several months later by systematic suspension. In one case the tenth, eleventh, and twelfth dorsal and first lumbar arches were removed. The dura was not opened.

At the upper portion of the wound the cord seemed to be diminished in consistency. The wound healed after slight suppuration, although some improvement followed. Dr. Dandridge thinks that it was due to the assiduous care and attention the patient received, and not to the operation. "In this connection it is a matter of interest and importance to determine after what length of time we must give up all hope of the disappearance of paraplegia in Pott's disease. I desire to place on record a case bearing on this point, in which after complete paralysis for more than three years and a half voluntary power returned, and six months later the child was able to walk with a slight support. The child, now 7 years old, has been under continuous observation for years at the Children's Hospital, Cincinnati, with marked curvature in the upper dorsal region. On admission there was complete loss of power in both lower extremities, but neither incontinence nor bed-sores. This condition continued without improvement until six months ago, when motion began to appear, at first in the thigh-muscles, and now he is able to stand, and with slight aid to walk. The improvement has been continuous, and complete restoration seems probable. For nearly two years the treatment has been little more than proper nourishment and good hygienic surroundings. The case has made a deep impression on my mind, as showing the power of recuperation possessed by the cord."*

Dr. Dunnavant reports (*Memphis Journal of the Medical Sciences*, 1889–90, vol. i. pp. 142–144) the case of a gunshot wound of the spinal cord in which the laminæ of the second and third dorsal vertebræ were shattered, and the cord itself completely severed. Operation was performed, and several spicula of bone removed. The patient died four days later.

Dr. Thomas H. Manley reports (*New York Medical Record*, 1890, vol. xviii. p. 202) the case of a fracture of the sixth cervical vertebra which was found on operation to involve the laminæ. The patient died at the end of the fourth day from consecutive hemorrhage. The cord at the autopsy showed no signs of traumatic or pathological disintegration.

Delorme reports (*Gazette des Hôpitaux*, 1890, vol. lxiii. p. 249) a case of wound of the base of the neck by a musket-ball. This was followed by suppuration, which lasted for seventeen years. Radiating pains were felt in the left upper extremity, and were localized in the left half of the neck. There was condensing ostitis of the lower portion of the vertebral column on the left side. Resection of the posterior extremities of the first four ribs on the left side was performed, and at the same time the spinous processes of the first dorsal and seventh cervical vertebræ and a portion of the laminæ and of the transverse processes of the latter vertebra were removed. A mass of callus was found in this region, and was removed at the same time. The patient was entirely relieved of the pains, which had previously been almost unendurable, and made a complete recovery.

Dr. L. S. Pilcher has reported ("Annals of Surgery," 1890, vol. xi. p. 194) a case of fracture of the spine at the junction of the dorsal and lumbar regions, followed by rapidly-ascending paraplegia, and by rigors and fever. On the eighth day the spines and laminæ of the last three dorsal and first lumbar vertebræ were removed, exposing a partly-organized blood-clot about a line in

* After a review of his own and other cases of fracture, Dr. Dandridge formulates the following provisional conclusions: "1. In fractures of the cervical vertebræ there is indicated immediate reduction of any displacement by extension and manipulation under an anæsthetic, followed by continuous extension and immobilization.* 2. In all fractures of the dorsal or lumbar spine involving the bodies or the arches, with or without evidence of injury to the cord, the immediate application of the plaster jacket by the hammock suspension preceded, if there is evident displacement by an extension, under an anæsthetic. 3. When symptoms indicating injury to the cord persist without improvement resection is indicated. 4. Immediate operation would be indicated where there is marked depression of the arches with symptoms of paralysis. 5. Long continuance of the symptoms is not in itself a contraindication to operation. 6. We have in suspension the means of alleviating some of the sequelæ of fracture of the spine."

* Dr. Herbert Burrell, in a valuable paper read before the Massachusetts Medical Society, June, 1887, has recorded the results of immediate rectification of the deformity and fixation of the spine by means of a plaster jacket. He details sixteen cases, in which three died, three derived no benefit from the method, and ten were greatly benefited. He arrives at the following conclusions:

1. That in the immediate correction of the deformity and fixation with the plaster-of-Paris jacket or other means we have a rational method of treating a large number of cases of fracture of the spine.

2. That, considering the hopelessness of results in fracture of the spine when treated expectantly, almost any risk is justifiable.

3. That the immediate correction of the deformity is imperative, as softening of the cord can and does occur from pressure at the end of forty-eight hours.

4. That the suspension of the patient is only a means of rectifying the deformity; that certain fractures could be simply pressed into position while the patient lies prone or supine.

The objections to the treatment are: 1. That the expectant plan of treatment gives a small percentage of recoveries. 2. That there are, especially in the cervical region, serious risks attending the suspension of a patient with a fractured spine and the rectification of the deformity,—in the way of shock, collapse, and death. 3. That in atempting to relieve pressure on the cord by rectifying the deformity, we might either sever the spinal cord or make pressure upon it. This is a matter of chance.

My own belief regarding the status which the procedure should occupy in surgery is, that it will occasionally be a life-saving measure; that it should be applied under anæsthesia in all cases of fracture of the spine which are not conclusively shown to be irremediable; and that, apart from the chance of recovery offered to the patient by this means, it will almost invariably make the patient more comfortable, in that he can be handled more easily.

thickness, lying external to the dura mater. The line of the fracture was through the dorso-lumbar articulation, with fracture of the spine and the laminæ of the last dorsal vertebra, but without depression or displacement. The patient died of asthenia thirty-six hours after the operation.*

Dr. Robert Abbé reports (*New York Medical Record*, 1890, vol. xxxiii. p. 85) three cases in which he operated for paraplegia from fracture. In one there was paraplegia below the eleventh dorsal of two and a half years' duration; the patient died thirty hours after operation. In another there was a fracture of the eleventh dorsal vertebra with paraplegia. At the operation the dura was opened. The patient recovered, but remained unimproved. In the third, three arches, the tenth, eleventh, and twelfth, were cut through and the spinal cord laid bare for two and one-half inches. The twelfth dorsal vertebra was found to have been displaced backward, the fracture running through the articular facets, the pedicles and laminæ. The cord was compressed between the arch of the eleventh above and the upper lip of the body of the twelfth below. The intervertebral cartilage had been ruptured. Above the line of pressure the cord pulsated, below it did not.

In half a minute after the cord was released from its flattened state (the bone-pressure area being only half an inch deep) the dura became rounded up as full as it was above or below. When it was filled out there was no surface appearance by which one might say that the cord within was not normal, except that the upper portion pulsated. Dr. Abbé emphasized this point because surgeons so often have reported that the cord appeared normal, and therefore they did not open the dura. The wound now being irrigated and dried, the dura was slit up for two inches. Adhesions of various densities were found within, attaching the meninges to the dura, and they formed a complete circular dam, shutting off the upper from the lower part of the canal. Only an ounce of clear spinal fluid came out, as the spinal canal had purposely been inclined on the table so as to prevent this. The veins of the cord were not distended. The adhesions were, with very little force, broken up. The cord was normal in thickness above the involved part, then, by a sloping rather than an abrupt change, it merged into a flattened cord for three-fourths of an inch, retaining its breadth but less than half its thickness, the principal atrophy seeming to be in the posterior columns. Throughout this flattened portion the white fasciculi of the cord could be traced continuously, so that there was no abrupt break in its continuity. The patient recovered from the operation, but remained unimproved.

Jaboulay reports the histories of two patients suffering with indirect fracture of the spinal column on whom he operated. In one there was a fracture of the sixth dorsal vertebra. The symptoms were extremely urgent and becoming more so. At the end of two days the arches of the fifth, sixth, and seventh dorsal vertebræ were raised, and the dura mater incised; a quantity of blood escaped from the sub-arachnoid space. The patient lived eighteen hours. The second patient, a woman 38 years of age, had a fracture in the dorso-lumbar region. She was paraplegic, had bed-sores, and had a fracture of both bones of one leg, which at the end of four months did not show the least trace of consolidation. The operation consisted in the removal of the arches of the twelfth dorsal and first and second lumbar vertebræ, together with a fragment which completely obstructed the lumen of the spinal canal. One month afterwards the œdema of the legs had disappeared and the fracture of the leg had healed, no apparatus having been applied. The patient died five months later. In both these cases the marrow was much softened at the level of the fracture (*Lyon Médical*, 1890, vol. lxiv. pp. 265–269).

Thorburn has reported ("A Contribution to the Surgery of the Spinal Cord," 1890) five cases of trephining for injury.

CASE I.—The patient, a man, aged 38 years, had a dislocation of the fifth cervical vertebra, with paraplegia, complete paralysis of the right upper limb, absence of reflexes, etc. The operation was performed about twenty-four hours after the injury. The laminæ of the fifth and sixth cervical vertebræ were removed by bone forceps. The dura mater appeared normal and was not opened. The patient died the following day. At the autopsy, the fifth cervical vertebra was found projecting slightly forward. No fracture was discovered. The dura was uninjured, but the cord was flattened opposite the seat of injury, and was much contused for about an inch above and below, containing hemorrhages in its

* Dr. Pilcher, after reviewing the objections to the operation, adds: "I am, however, far from accepting the sweeping condemnation with which Jonathan Hutchinson and Le Gros Clark have declared all such operations unjustifiable. The reasons which prompted me to operate in the one case in which I did interfere I believe to have been valid and sufficient, notwithstanding the fact that no special benefit accrued to the patient from the attempt. Fractures of the spine in which the lesion is caused by a force which acts only upon a very limited segment of the column evidently form a class much more likely to present conditions amenable to operative interference than cases in which the injuring force is more diffused. The injury to the cord will be much more limited in extent; the presence of depressed fragments pressing into or upon a wounded cord is more likely to exist; no extensive resection of the posterior arch of the canal will be required. And those cases in which the bone injury is limited to the spinous processes and laminæ form a class which should be distinguished clearly, as regards the possibilities of advantage by operative interference, from the cases in which there is complete fracture through the bodies of the vertebræ. As is well understood, in many cases of complete fracture—illustrated in the two cases above detailed—the soft structures which bind the bones together posteriorly are not ruptured, and continue to form a most important bond to prevent displacements in the subsequent history of the case. To divide these, as is necessary if the spinal canal is to be opened, entails the destruction of this means of support which cannot be adequately substituted, and is a sacrifice which ought to be made with great caution and only on the clearest evidence of the existence of a more imperative indication. If, however, the bodies are intact, or if the case has progressed to a period in which consolidation of any fracture involving them has occurred, then the posterior arch may be attacked without seriously increasing the hazards of the case. Gunshot fractures of the vertebræ form yet another class, one in which there is the special indication arising from a compound fracture, probably septically infected, for immediate enlargement of external wound, removal of fragments of bone, disinfection, and drainage" ("Annals of Surgery," 1890, vol. xi. pp. 196, 197).

substance and in the central canal; elsewhere its structure was normal. The first and second bones of the sternum were also partially separated, and the lungs much congested.

CASE II.—A man, aged 33 years, had a fracture of the body of the fifth cervical vertebra which projected backward, distinctly compressing the cord. He was operated upon five days after the injury. A fracture of the fifth cervical spinous process was found so loose that it could be wrenched away in one piece with the left lamina. When this fragment of bone had been taken away the dura was partly exposed, and the right lamina of the sixth cervical arch was also found to be fractured. The left lamina of the sixth arch was then divided and the whole posterior portion of the arch removed. The exposed dura was normal, and was obviously free from compression. During the operation respiration ceased, and the patient's condition became very critical, requiring him to be turned on his back for a time, with the use of artificial respiration and inhalation of nitrite of amyl. The result showed that the spinal cord was here exposed to continued pressure from the displaced bones, and that this pressure must have been relieved by the removal of the counter-pressure due to the excised arches. In spite of this, not the slightest improvement resulted in the symptoms, and it seems not improbable that the operation may have shortened life somewhat, owing to further damage having been inflicted upon the compressed cord by the necessary manipulations, especially during the period of suspended respiration. The patient survived the operation forty-eight hours.

CASE III.—A man, aged 39 years, after a severe injury, had marked immediate paralysis of sensation and motion in the trunk and limbs, with increased reflexes and anæsthesia extending far up the trunk. He improved for five weeks, after which the case became stationary, except that his general health was affected by a grave cystitis. Fifty-six days after the accident he was, therefore, operated upon. The arches of the fifth and sixth cervical vertebræ were removed. The posterior aspect of the dura was smooth and normal, but the membranes projected somewhat backward, as if distended. The patient died with high temperature and coma eight days later. At the autopsy it was found that the cartilages between the second and third and the third and fourth cervical vertebræ were broken, and there had evidently been severe over-flexion in this region, causing some compression of the upper anterior edge of the body of the third cervical vertebra, but no fracture. The bones had then returned to their positions, leaving the spinal canal free from any permanent compression. The spinal membranes were much congested through their whole extent. The cord itself was soft and diffluent opposite the fourth, fifth, and sixth cervical vertebræ. From these appearances it will be obvious that there had been over-flexion of the cervical region of the spine, and that although the greatest damage to the vertebral column was situated opposite the third cervical vertebra, the cord had suffered mainly from hæmatomyelia opposite the fifth.

CASE IV.—A boy, 15 years of age, with displacement forward of the second lumbar vertebra and compression of the cauda equina. There were paresis of the lower limbs, diminution of sensation, coldness of feet, etc. About four months after the accident he was operated upon. It was found that the arch of the second lumbar vertebra was displaced forward. This was almost completely removed by bone-forceps, exposing the membranes of the cord, which had obviously been compressed by it. Mr. Thorburn remarks that this case differed materially from the last three, both in the nature of the injury and in the result. The injury did not affect the spinal cord at all, but was confined to the cauda equina, and an interval of four months and a half had elapsed between its receipt and the date of the operation. During the earlier part of this period the symptoms had improved, but, as in the last case, this improvement had ceased, and for some eight weeks there had been no change in the patient's condition. The operation revealed a large amount of cicatricial tissue compressing the nerves of the cauda equina, and on the removal of this tissue improvement recommenced, and was eventually very satisfactory.

CASE V.—A man, aged 28 years, had a dislocation of the last dorsal vertebra, with paraplegia and partial anæsthesia. After slight temporary improvement, the case became stationary. Operation was performed eighteen weeks after the accident. The arch of the first lumbar vertebra was removed, so as to lay bare the dura. The latter appeared to be healthy, although protruding somewhat backward, but between the arches of the first lumbar and the last dorsal vertebra it was compressed by a mass of soft cicatricial tissue. As much as possible of this tissue was removed, together with the whole of the arch of the last dorsal vertebra, and the theca was then found to be so much flattened that its contents appeared to have been quite cut across. Immediately above this region the dura mater bulged slightly, and, a small spicule of bone having penetrated it, some cerebro-spinal fluid escaped. All pressure on the theca being now removed, the skin-flap was replaced and sutured. As in the last case, cicatricial tissue was found surrounding the nervous structures, and this tissue was removed, besides which the cord itself appeared to be almost entirely torn across. The subsequent improvement was practically *nil*, except for a slight recovery of function in the upper lumbar roots, which, having left the cord above the seat of the lesion, form the commencement of the cauda equina.

Mr. Golding-Bird reports (*British Medical Journal*, May 23, 1891) a case of paraplegia from fracture in which he removed the depressed laminæ of the eleventh and twelfth dorsal vertebræ and a large extra-medullary clot. He operated on the third day after the injury. The patient made a complete recovery, sitting up in bed on the eighteenth day. Mr. Golding-Bird makes the following interesting remarks in reference to the case:

"As in compound fractures of the cranial vault there is a marked difference in respect to treatment between one produced by a heavy blow and another by a sharp-pointed instrument, so in fractures of the spine the line of treatment to be adopted must be determined largely by the manner in which the fracture has been produced. When it can be ascertained that it is due to direct violence, as in this case, it has always seemed to me (and this case bears this view out) that we ought to cut down and explore for bone (lamina) driven on to the cord, just as in a puncture through the skull we always trephine for fragments of the inner table, which are often extensive when the outer wound is quite insignificant. But such cases of direct fracture are not so common as those where, falling from a height, the patient alights on the lower part of the body, breaking the spine higher up, and in such a way that the total upper part overshoots the lower, cutting the cord in two as though by a pair of scissors. In such cases operation can hardly

be expected to be of service. But often the history given of the accident is so obscure, and the result of an examination so uncertain, that it becomes a matter of pure speculation whether the paralytic symptoms are due to pressure by a broken arch or by blood-clot, or to division of the cord by a fracture of the vertebral bodies. Cases of spinal fracture are in their results some of the most sad and disappointing that can be met with in practice, for, even when life is saved, the after-condition of the patient is, to say the least of it, almost always most unsatisfactory. Seeing, then, the doubt that usually surrounds the precise nature of the fracture, and knowing what an unfavorable future lies before the patient, a surgeon is surely justified in every case in urging upon the patient the propriety of an exploratory operation, in the hope that the condition found on exposure of the bone may be a relievable one.

"Laminectomy is a recognized operation, and a perfectly safe one, whilst for its performance no special instruments are required, though special forceps for cutting the laminæ are preferred by some surgeons. This case shows that even two consecutive neural arches may be completely removed without the slightest detriment to the stability of the spinal column. That the patient's paralytic symptoms were not due to 'shock,' but to actual pressure on the cord (and therefore likely to have been lasting had no operation been performed), is shown by the fact that the left side was the worse, and it was two pieces of the left lamina of the twelfth vertebra that overlay each other and compressed the cord. A further argument for laminectomy in spinal fracture is a physiological one, and it indicates the time when the operation should be performed. In limited depressed simple fracture of the skull, though symptoms of compression are present, complete recovery may be seen though the bone is not elevated; but that is no argument for leaving a depressed lamina alone. In the case of the brain, the pressure is exerted over gray ganglionic matter, of which limited parts may be destroyed without permanent loss of function in the periphery over which that ganglionic mass presides; but the spinal cord is mainly a mass of conducting fibres, the destruction of even a minute area of which represents a large area of brain matter 'thrown out of circuit' and rendered permanently useless; hence a degree of displacement of a neural arch appreciable to the examining hand of the surgeon is no criterion of the degree or extent of harmful pressure upon the cord. Inasmuch as nerve-fibres, when divided, begin within three days to degenerate, laminectomy for spinal fracture should be undertaken within that period; for though paralysis from pressure of the cord may exist for much longer and yet be recovered from, thus showing the pressure to have been not too destructive of the fibres of the cord, yet if we wait long enough to judge of the severity of the lesion by the disappearance or not of the paralytic symptoms, we shall certainly have waited too long for operation to offer a chance of success if the latter of these results unfortunately supervenes." *

Dr. D. N. Knox reports (*Glasgow Medical Journal*, June, 1891) a case in which he trephined for fracture-dislocation of the eleventh dorsal vertebra, which was displaced backward fully three-fourths of an inch, and rotated as much to the right side. The operation was performed thirty-six hours after the injury. The patient was a boy of 13. The laminæ of the tenth and eleventh were removed. The dislocated vertebra was replaced. Sensation and motion were both completely lost before the operation, but the former returned the following day, and on the third day he regained slight motion of the toes. The progress of the patient was steadily but slowly towards entire recovery.

In eighty cases we find that there were thirty-three recoveries from the operation itself, and forty-seven deaths,—a mortality of about fifty-eight per cent. The older statistics were still more unfavorable, because, first, the cases selected were of the most extreme gravity, and, secondly, trephining opened the way for bony or meningeal infection. The want of success, even since the opening of the antiseptic era, has been due to somewhat the same causes. Pinkerton's case died of operative infection, with a temperature of an acute meningomyelitis. Keetley's had a cervical lesion, and died with pulmonary complications, after having improved for some days. Duncan's case was found at the autopsy to have a diaphragmatic hernia of the stomach and omentum, the result of the severity of the accident. Allingham's case died at the end of seven months, and the cord was found to be completely divided. One of the three patients unsuccessfully operated on by Thorburn had a cervical fracture, with anæsthesia, paralysis of all four limbs, retention of urine and fæces, and very extensive ulceration. He died in forty eight hours. The second had a fracture in the same region, and of even graver character, and died in twelve hours. The third had also a cervical lesion, and died in eight days, with a high temperature and a suppurating wound. The two patients of Jaboulay were desperate cases at the time of operation.

In those who survived, a varying degree of success was obtained. Werner, writing of seven cases of recoveries of which he knew, and eliminating the case of Blair, which is very insufficiently described, adds: "Trephining in none of the six remaining cases restored the use of the lower limbs to the patients. In the most favorable, feeble motion in the arms and thighs returned, and the sitting position became possible. In all of them sensation returned more quickly and completely than motion. In nearly all of them the vesical functions, and in one case the rectal, were restored."

One patient, that of Edwards, had two children after the trephining. Very recent operations have been somewhat more successful. In the case of Horsley, sensation improved alone.

* In concluding this review of the history of operations for spinal traumatisms, I may summarize a portion of another exhaustive article by M. Chipault (*Gazette des Hôpitaux*, 1890, vol. lxiii. pp. 809, 969, 983).

In that of Allingham, paralysis descended to the level of the umbilicus. In that of Dawbarn there was slight improvement in motion and in the rectal and vesical functions. Dandridge and Deces had similar results. Thorburn had one incomplete success, and one so satisfactory that at the end of a year and a half his patient earned his living as a miner and was able to walk many miles daily. Macewen's patient walked without a cane a year after the operation. Lauenstein's patient at the end of six months could walk all day without a cane, and go up and down stairs easily. Golding-Bird's patient was able to sit up in bed, and apparently was quite well again in eighteen days. Knox's patient regained sensation in twenty-four hours, and a little motion in three days, and was apparently on the road to entire recovery.

The indications and contraindications for trephining in special fractures are, according to Chipault, based on the following points: 1, *the nature of the vertebral lesion and the nature and extent of the medullary lesion*; 2, *the time which has elapsed since the traumatism*; 3, *the regional level of the medullary lesion.* In studying these points only those cases should be included in which the lesion and the operation have been at a higher level than the first or second lumbar, as below that point there are only the bundles of nerve-fibres which constitute the termination of the cord and the cord itself. 1. *As to the nature of the spinal and cord lesions.* *a.* Of course the spine may be fractured without injury to the cord. In the absence of all symptoms, no one would think of operating. *b.* If the arches alone are fractured, operation is clearly indicated. *c.* The propriety of operation is much more doubtful when the spinal lesion affects the bodies also or even the bodies alone. Felizet and other partisans of operating have said that in these cases the cord is compressed between the anterior face of the arch and the posterior face of the fragment of the body; and that if the cord is severed death is certain at any rate, while if it is not severed operation may result favorably. Chedevergne, on the contrary, believes that the cord is so fixed by the attachments of the pia mater that it is wounded or divided by the anterior fragment, but cannot be compressed between that fragment and the arch. Menard denies this fixity in a study of the mechanism of indirect fracture of the vertebral column in the dorsal and dorso-lumbar regions.

Thorburn has divided vertebral fractures into three classes: 1, those in which after death we do not find the canal notably narrowed, the displaced bones after the accident having resumed their proper position, leaving the cord contused, or even pulpefied, but free from pressure,—these cases constituting the majority; 2, those in which the displacement is permanent and the cord is compressed; 3, those rarer cases in which pressure upon the cord is due to intraspinal hemorrhage.

In the first of these groups, it must be admitted, all intervention is useless. In the third group, that of hemorrhage into the canal, operation would be of great advantage, but such hemorrhages are very difficult to diagnose. In the second group, which according to many surgeons is the most frequent, Thorburn believes that cure is scarcely possible, as he thinks that the clinical testimony shows that the damage done by acute compression of the cord is usually irreparable. This is contradicted to a certain extent by the experiments of Brown-Séquard and others already quoted. Certainly, however, the most favorable cases are those in which the cord is only partially destroyed or slightly altered: so that the two great questions which present themselves to an operator in the presence of a fracture of the spine are: 1, Is the cord now compressed by a fragment of bone? 2, has the cord been completely destroyed, partially destroyed, or simply contused?

In regard to the first question, we should examine attentively the deformity, and the relation of the posterior arches and of the articular processes, and can sometimes, but by no means always, arrive at a correct conclusion. As to the second question, the complete disappearance of both sensation and motion below the lesion would indicate the complete transverse destruction of the cord. The disappearance of either one or the other alone, especially when localized on one side or at certain points, indicates a partial destruction. It will be understood, of course, that such localized or partial paralyses after fracture are extremely rare. When they do exist, however, they constitute a valuable guide to the operator.

MacDonald thought that a disappearance of sensation less complete than that of motility indicated medullary compression without destruction. Tillaux thought that the preservation of the muscular sense indicated simple compression. The condition of the reflexes is one of the most important elements in the problem. Tillaux says that while the exaltation of the reflex has been considered as a sign of destruction of the dorsal or cervical cord, he believes that in cases of fracture in the lumbar region its absolute disappearance would generally be a sign of such destruction, because although the cord acts with more vigor

beneath the cervical lesion, it could not do the same in the lumbar region, since beneath that there is no cord, so that in lumbar fractures the persistence of reflex action is the favorable sign and its abolition the unfavorable one. Bastian and Bowlby have recently investigated the same subject. The former found that in two cases of fracture of the spine in the lower cervical and upper dorsal regions there was complete abolition of reflexes; but at the autopsy cord lesions were limited to the level of the traumatism, and the lumbar region presented no change except the secondary degeneration. Bowlby says that he has observed twenty-two cases of cervico-dorsal fracture. In two the reflexes were exaggerated, and the cord was not completely destroyed. In the twenty other cases, of which fifteen were verified by autopsy, and in which the transverse destruction of the cord was complete, the reflexes were completely destroyed, even in one case where the patient lived for ten months.

According to this, therefore, the complete disappearance of reflexes below the lesion would be a valuable sign of complete transverse destruction of the cord.

2. As to the time between the accident and operation, both experimental and clinical facts show that the degenerative changes which are set up after a violent contusion or laceration of the cord take place with such rapidity that if *much* time has elapsed a large proportion of cases become hopeless. On the other hand, Horsley's case of tumor, and many cases of recovery from Pott's paralysis, with and without operation, show unmistakably that even long-continued pressure, almost to the point of obliteration, may be followed by a return to normal conditions.

3. As to the region involved, both theoretical and statistical considerations show that the lower dorsal and lumbar vertebræ are those which can be operated upon with the greatest hope of success. As to trephining in fractures below the level of the cord, that is to say, below the first lumbar vertebra, the results are, as might be expected, especially satisfactory. In looking at the statistics collected by Chipault, we find in fifty-six cases fifteen cures, of which two were complete (Péan and Lauenstein): the case of M. Péan was, however, a fracture of the posterior arch only. In three of the remaining thirteen (Blair, and Stemen, two) the seat of the lesion is not stated. The case of Alban Smith is without value, on account of carelessness in reporting. In three others (Walker, Potter, and Allingham), the lesions were seated about the lumbar region. In the case of Walker, however, the operation was done on the day following the accident, and it is not certain that a spontaneous cure might not have occurred. There was improvement in the cases of Edwards, Blackman, Gordon, Macewen, Lauenstein, Horsley, and Dawbarn, in which the lesion affected respectively one of the lumbar vertebræ, the sacrum, the twelfth dorsal or first lumbar, the twelfth dorsal, the junction of the twelfth dorsal with the first lumbar, the eleventh dorsal, the junction of the last two dorsal. In these seven cases the filum terminale was certainly affected in the first two. In the third, the terminal portion of the cord was doubtless injured, and the vesical and rectal centres did not participate in the improvement. The same remark applies to the fourth case. In the fifth, the reparative process appeared to have taken place in the cord itself. In the sixth, the improvement was associated with reparation of the nerves of the filum terminale, and not of the cord. In the seventh, the improvement was scarcely noticeable. Thorburn's rule is deserving of consideration in this connection, but should be contrasted with the remarks of Mr. Golding-Bird apropos of his case of fracture. Thorburn says that for the filum as for peripheral nerves the chances of cure do not seem notably diminished by expectant treatment. Experience shows, he says, that if a spontaneous cure is to take place it will begin early and will be progressive, and he thinks that, therefore, if at the end of six weeks there is no improvement, or if the improvement ceases, operation is indicated only then. In only one of his five cases did he have a complete cure, and then the lesion was confined to the filum, and a considerable quantity of cicatricial tissue compressing its nerves was removed.

I believe I have now given a fairly complete view of the history of this operation in cases of fracture and luxation, and with sufficient detail to permit of a reasonable estimate of its present position before the profession.

The arguments employed against operation in these cases of pressure upon the cord from fracture or displacement may be summarized as follows. It was said that there was great difficulty of diagnosis between the cases in which the operation would probably prove useless and those in which it would probably afford relief on account of the limited extent of the injury. This is undoubtedly true, but a precisely similar argument will apply to many other serious and frequently-employed operations, and all the most recent additions to our knowledge of the subject, whether derived from clinical experiments or from experimental and pathological investigation, lead us to assign to the cord a much greater power of repairing damage than was formerly thought to be possible.

It was said that the position of the fractured

bone often rendered it impossible to reach it, and that the pressure was commonly from the bodies of the vertebræ which could not be removed. A careful review of reported autopsies shows that the proportion of cases in which the pressure is from the laminæ is greater than has been supposed, and that this assertion is as true now as when originally made by Mr. Nunneley twenty years ago.

It is easy to demonstrate that the too sweeping statements which have been quoted, as to the extreme rarity of direct compression by the arches and spinous processes, are not fully justified by the facts. I append a number of references to cases of this character, selected almost at random from medical literature, while seeking information as to other points. They might be greatly multiplied.* They demonstrate that, in addition to the relief which we may perhaps expect to give by removal of the laminæ even in cases of compression by posterior displacement of the bodies, we may expect, in a proportion larger than has been generally supposed, to remove completely and permanently the actual compressing and irritating factor, thus giving the cord an opportunity for reparative action if it has not been hopelessly crushed or disorganized,—an opportunity which could not occur if the patient were left to the unaided processes of nature.

Of the cases of resection for fracture and luxation which have thus far been recorded, I find that in no less than forty-five per cent. was there a fracture of the laminæ or processes,—a percentage which, while it is undoubtedly far in excess of the actual frequency of this form of fracture, should yet be studied in the light of the statement made by the opponent of the operation,—viz., that it is impossible to tell with accuracy the extent of the damage and the particular parts involved. If this impossibility really exists, it is all the more important that we should not lose sight of the fact that the injury *may* be one quite relievable by operation.

Even when the pressure is anterior, having been caused by displacement or fracture of the

* Perhaps the following case, recorded by a distinguished neurologist, will serve as well as any as an example of a group of cases which a very little research shows to be by no means infrequent.

Dr. Charles K. Mills has reported (*The Polyclinic*, vol. i., No. 9, 1884) the case of a man who had been crushed by the roof of a building falling upon him; he had motor and sensory paralysis, never recovered, and died of exhaustion five years later.

A post-mortem examination was made thirty hours after death by Dr. Flick. The vertebral column had been fractured at the position of the twelfth dorsal and first lumbar vertebræ. The posterior portion of the upper of these bones had been broken and driven inward, and its fragments were overridden by a portion of the lower injured vertebra. The dura mater was adherent to the bone at the seat of the fracture. In the external layer of the dura, just at the lower end of the spinal cord, was a bony mass, one-half inch long by one-fourth inch wide. The membrane, for a short distance above this point and down to the cauda equina, was much thickened and covered with a dirty exudation. The cord at this point was red and very soft. The spinal canal contained a large effusion.

Dr. Mills, in reporting this case, said, "This mass which was like the bony concretions sometimes found in the falx cerebri, could have been removed by operation, after elevation of the depressed and broken spinal arch."

He adds, "In another case operation might have been of benefit if performed immediately after the accident. The most important features of this and the first case were as follows. A history of severe injury to the middle and lower spine; this was followed at once by pains in the back and limbs, spinal rigidity, anæsthesia, and paralysis of the lower limbs and of the bladder and bowels. The condition remaining was one of motor paralysis, wasting, contraction, depressed electro-contractility, coldness and lividity, peculiar zones of anæsthesia, and abolition of the tendon reflexes in anæsthetic regions. In this second case, at the upper edge of the twelfth dorsal vertebra adhesions were found which continued down the canal for three or more inches. At the commencement of the cauda equina, which was about the centre of the adhesion, caries of the first lumbar vertebra was found, the portion involved being the inner surface of the laminæ close to the junction of the body. Above and below the caries the adhesions were most persistent. The conus medullaris, cauda equina, and membranes were a glued mass; for an inch or two above and below this the membrane was considerably thickened, a deposit being present of a calcareous nature ("Spinal Localization," THERAPEUTIC GAZETTE, May 15 and June 15, 1889).

Cases more or less similar, but agreeing as to the occurrence of fracture of accessible portions of the vertebræ, have been reported by Astley Cooper; Hamilton, "Fractures and Dislocations;" Stephen Smith, *N. Y. Journal of Medicine*, vol. vi., 1859; Curling, two cases, "London Hospital Reports," vol. i.; Van Dyck, *N. Y. Journal of Medicine*, vol. iii., 1857; Eberman, *Am. Journ. of Med. Sci.*, October, 1879; Arnott, *Lancet*, 1851; Bransby Cooper, *Lancet*, 1828; Williamson, *Dublin Quarterly Journal*, vol. xxvii.; Boyer, "Surgical Works," vol. ii.; C. S. May, *Am. Journ. of Med. Sci.*, October, 1876; Erichsen, "Science and Art of Surgery;" Malgaigne, "Treatise on Fractures" (Packard's translation), *N. Y. Journ. of Med. and Collat. Sci.*, September, 1849, p. 268; Hamilton, "Treatise on Fractures and Dislocations;" "Guy's Hospital Reports," vol. ii. p. 480; F. H. Hamilton, Jr., *American Medical Times*, N. S., vol. viii.; Berkeley Hill, *Trans. Royal Med. and Chirurg. Soc.*, January 22, 1867; Abernethy; J. Comstock, *Boston Medical Journal*, 1848; William Pepper, *Trans. Patholog. Soc. of Phila.*, 1867; T. B. Ladd, *Boston Medical Journal*, 1852; A. P. Clarke, *Journal of the Am. Med. Association*, October, 1884; S. D. Townsend, *Am. Journ. Med. Sci.*, O. S., vol. xxii.; J. Solis Cohen, *Trans. Patholog. Soc. of Phila.*, 1878.

vertebral body, removal of the posterior arches increases the calibre of the canal, and increases also the chance of avoiding irreparable damage to the cord by compression. That it is unsafe to say in any given case that the cord is *hopelessly* damaged would seem to be fairly well established by the results of such cases as Horsley's, Macewen's, and Lauenstein's, so that operation should scarcely be refused on that score alone.*

A still further argument was that the exposure of the cord and membranes to the air would inevitably set up a destructive inflammation, an argument which in these days of antiseptic surgery need not further be considered. The general conclusions that the operation was too difficult, was too prolonged, that there was danger of fatal hemorrhage, etc., were, as experience has shown, largely theoretical; and although the operation is certainly not an easy one, and although one operator did lose his patient on the table from hemorrhage while attempting to perform it, it is still one which may be confidently undertaken by any experienced surgeon.

In reviewing the statistics of these operative cases, it seems to me quite proper that those belonging to the pre-antiseptic period should be omitted, or should be regarded in a category by themselves. I do not mean that all, or even a majority, of the fatal cases were due to septic disease. In many of them the time was too short, in others death resulted from intercurrent disease of other organs; but a sufficiently large proportion were so distinctly septic as to invalidate any sweeping conclusions based upon the results, sepsis after this operation being as absolutely avoidable as after any other in surgery. This would leave, then, in a class by themselves, the thirty-six operations for fracture of Macewen, Morris, Horsley (two), Lauenstein, Lucke, Albert, Duncan (three), Péan, Allingham (two), Dawbarn, Thorburn (five), Dandridge, Deces, Jaboulay (two), Abbé (three), Pilcher, Manley, Bell (two), Armstrong, Halstead, Pinkerton, Golding-Bird, Richardson, Knox, with six complete recoveries, five recoveries from the operation with benefit, eleven recoveries unimproved, and fourteen deaths,—a mortality of thirty-nine per cent.

The chief strength of the opponents of operation lies in the argument that the operation *per se* is of great danger, or in itself materially diminishes the patient's chances. If, as I believe to be the case, it can be fairly claimed that rapid reunion of all the soft structures down to the dura mater itself can be confidently expected; if it can be shown, as it has been, that extensive resections of the laminæ do not greatly or permanently weaken the spine; if under antiseptic methods the risk of consecutive inflammation of cord or membranes is practically *nil;* if hemorrhage is not to be feared, and if loss of cerebro-spinal fluid is unimportant; if it happens not so very infrequently that the cord is directly compressed by fragments of the laminæ themselves, or, if not, that by removal of the arches relief from anterior pressure *may* be afforded,—if these are facts, or even reasonably strong probabilities, it is evident that the operation is one which should

* In his article in "Holmes's Surgery" (vol. iv. p. 130), Alexander Shaw says, "Changes of the most extraordinary magnitude may be effected in the brain and cord by encroachments of various kinds, without the sacrifice of their functions, on condition that the intrusion is made slowly and gradually." This opinion has been reiterated even by surgeons who, like Mr. Shaw, earnestly oppose resection, and is used as an argument for trusting the patient to the *vis medicatrix naturæ*. Professor Eve has brought together (*Amer. Journ. of the Med. Sciences*, July, 1868) eleven cases of supposed division of the spinal cord in which life was variously prolonged, and in one of which recovery was complete. In the lower animals the results of vivisection experiments have been somewhat contradictory, but on the whole sustain the view that the cord, like other nerve-trunks, possesses extraordinary regenerative power. In some animals true nervous matter is introduced after part of the cord has been destroyed; at least this is so in tritons and lizards (Müller). As is well known, in these animals when the tail is removed it is reproduced, and Müller found that a part of the spinal cord corresponding to the new part of the tail is reproduced. Morphologically the elements were the same, but the spinal nerves were not reproduced, while physiologically the new nerve-substance was not functionally active. It corresponds, as it were, to a lower stage of development.

According to Masius and Vaulain, an excised portion of the spinal cord of a frog is reproduced after six months; while Brown-Séquard maintains that reunion of the divided surfaces of the cord takes place in pigeons after six to fifteen months. A partial reunion is asserted to occur in dogs by Dentan, Nannyn, and Eichhorst, although Schieferdecker obtained only negative results, the divided ends being united merely by connective tissue (Schwalbe).

Eichhorst and Nannyn found that in young dogs, whose spinal cord was divided—not excised—between the dorsal and lumbar regions, there was an anatomical and physiological regeneration to such an extent that voluntary movements could be executed. Vaulain in the case of frogs, and Masius in that of dogs, found motility restored first and sensation afterwards.

In 1882, Dr. Carl Maydl, of Vienna, conceived the project of resecting the whole thickness of the cord, and, after some successful experiments on dogs, actually opened the spinal canal of a man for that purpose, but found the conditions too unfavorable to proceed further. The patient recovered from the operation, but was not improved at all.

no longer be rejected on the sole remaining ground that we cannot be certain in any given case as to the exact amount of damage which has been done to the tissues of the cord. The argument that, if such damage were irreparable, operative interference would be useless, while if the cord retained the power of recovering itself the operation would only add another complication, has lost nearly all its force. It would seem rather to be the duty of the surgeon, after a reasonable and not very protracted delay, to endeavor to relieve any possible pressure, to remove any fragments or spiculæ of bone, to drain thoroughly the canal or even the subdural space if there be any oozing, and to do so with the consciousness that, if he meet with none of these conditions, he is at any rate not performing a necessarily fatal operation.

Chipault comes to the following conclusions: 1. Trephining is useless when, the cord having been contused, the fragments displaced at the time of the traumatism have returned to their normal position. 2. Trephining is almost certainly useful (*a*) when the cord is compressed between a vertebral body and a posterior arch; (*b*) when there is medullary compression from spinal hemorrhage. In these two conditions the least possible delay is advisable, on account of the extreme rapidity with which secondary degenerations occur.*

3. Trephining is absolutely indicated (*a*) when the symptoms are due to compression of the cord by fragments of the arches or laminæ; we should then operate as early as possible; (*b*) when there is compression of the filum terminale from any cause,—that is, from either anterior or posterior fracture or from the presence of cicatricial tissue. We should operate at the end of the same month if improvement has ceased for a few weeks.

Unquestionably, if for any of the above reasons we decide to operate, the earlier the operation is done the greater the probability that it will be of benefit. In the presence of depression of arches or laminæ, immediate interference is justifiable, as it is in cases with recognizable symptoms of spinal hemorrhage. In the absence of these conditions, and weighing fairly the fact that even in recent times and in good hands there have been but seventeen per cent. of complete recoveries and a mortality of thirty-nine per cent. after this operation, the safest rule with which I am familiar is that which has been formulated by Lauenstein,—namely, that if after the lapse of from six to ten weeks there is incontinence of urine, with cystitis or incontinence of fæces, and especially if there is also the development of bed-sores, little is to be hoped for from the unaided efforts of nature. If, however, these these symptoms be absent, and if there be the least improvement in either sensation or motion, it will be proper for the surgeon to delay operative interference still longer.†

* "I might give many illustrations gathered from medical and surgical literature of intraspinal hemorrhage in which operation would probably have proved successful, but I will confine myself to two, one twenty-two years old and the other a recent case.

"Jackson (*Lancet*, July 3, 1869) reported an interesting case of localized spinal apoplexy, which with our present lights might perhaps have been relieved and a life saved by careful trephining. The patient was a bright girl, 14 years old. While dressing, her fingers felt weak. The next day she had a similar weak feeling in her hands. One day later she was unable to move her arms except at the wrists. Later, the intercostal muscles did not act quite freely, and she seemed to lie heavier in the bed. Moist crepitant râles, with a little cough, developed. On the fifth day after the first symptoms, careful examination clearly demonstrated great loss of power in all the voluntary muscles of respiration and in those muscles of the arm, back, and chest supplied by the cervical nerves. The diaphragm became fixed, and there was slight lividity about the cheeks, with a fall in the natural temperature. Sir W. Jenner, who was called in consultation, diagnosticated a clot in the cervical portion of the spinal cord. The whole cervical portion of the spine, but particularly in front and to the left, was embedded in an oblong clot of dark venous blood outside the membranes. The clot ceased at the seventh cervical vertebra.

"In this case, as the reporter remarked, the effusion probably took place very gradually, had room to extend itself, and coagulated very slowly and imperfectly. Until the phrenic nerves were interfered with, nearly every symptom might have been attributed to hysteria.

"The following case was reported by Dr. Arlidge to the Staffordshire branch of the British Medical Association (*Med. and Surg. Rep.*, March 23, 1889, vol. lx., No. 12, p. 370). The patient, 44 years old, a drinking man, was admitted to the infirmary with complete motor paralysis of the legs and some weakness in the arms, where, too, formication and numbness were experienced. Sensation was normal, and the legs were painless and their reflexes abolished. His only complaint was of dorsal pain. The bladder was distended, and prior to admission had been emptied by the catheter, but there was no dribbling of urine. Some bronchial râles were noted in the upper part of the left lung; heart healthy; bowels confined for four days; the cerebrum undisturbed. The first temperature taken was 103.6°, but the next day it fluctuated between 105° and 106°, and rose to 107.4° the day preceding his death, and was accompanied by delirium. A post-mortem examination revealed a copious hemorrhage within the meninges of the spinal cord, extending downward from the last cervical vertebra for about six inches. The blood was coagulated. No ruptured vessels were found in the cord itself, and no cerebral disease, although the dura mater was very adherent to the skull. There was no fracture of the spine, and no caries of any vertebra" (Chas. K. Mills, "Spinal Localization," THERAPEUTIC GAZETTE, May 15 and June 15, 1889).

† All operators upon cases of fracture paraplegia of any duration have thus far arrived at about the same conclusion,—namely, that the pressure of bone is of secondary importance, except the fracture involves only

In conclusion, I desire to speak briefly of the operation itself, which has been variously described as "formidable," "appalling," "well-nigh impossible," "desperate and blind," and in other similar extravagant terms. In one case the patient is said to have died on the table from hemorrhage; in another the operation was abandoned on account of the supposed impossibility of its completion. It has not seemed to me that these descriptions are in the least justifiable. The operation is, indeed, as was said by McDonnell, apt to be tedious and tiresome; but, so far as any major difficulties are concerned, it is not to be compared with many much better known, and much more frequently practised, operative procedures. I base this opinion both upon experiments on the cadaver, and upon my experience with the cases in which I have performed resection. It may be useful to give the details of the method which seems to be preferable:

The patient having been placed in the prone position, and a gentle curve having been given the spine by means of a firm, small pillow placed under the lower ribs, a long incision should be made directly down to the tips of the spinous processes, the middle of the incision being opposite the supposed site of the tumor, the apex of the angle, or curve, if the case be one of Pott's disease, or the deformity, or displacement, if the case be one of fracture or luxation. The muscles should then be freely separated from the sides of the spinous processes and the posterior surfaces of the laminæ, the edge of a stout, thick-backed scalpel being used for this purpose. One side of the spine should be cleared, and all hemorrhage arrested by the use of hæmostatic forceps, before the other side is approached, and the wound should be filled with sponges, or a towel wrung out of a hot antiseptic solution. This having been done, the other side may be treated in the same manner. By the time the spine is thoroughly exposed on the second side, most of the bleeding will have permanently stopped on the other; but any distinct spirts or free oozing following the removal of the catch-forceps should then be permanently arrested by ligature, after which the periosteum may be reflected by making a firm incision through it along the angle between the spinous processes and the laminæ, turning up its edge at this point with the help of dissecting forceps, and then scraping the surfaces of the vertebral arches with a curved periosteal elevator. This being completed, the hemorrhage should be similarly arrested, and the perios-

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the arch, where it is driven in by a blow, inasmuch as the violence, usually a fall and bending of the back, which will produce instant paralysis, has done so by a diastasis of the vertebræ, the cartilages being ruptured and the arches broken, which completely pulpifies the spinal medulla. The vertebræ are very apt to resume immediately their usual relations. If, however, the fracture takes place at or below the last dorsal, where the medulla has disappeared and the firm cauda equina commences, the crushing does not usually destroy the nerves, but long bone-pressure would do so. In such cases operation to correct it is always desirable. It still remains a problem, perhaps never to be solved, how to connect the lower segment of the cord with the upper when there is a gap of half an inch, and whether this union would restore functional connection with the brain, even though its reflex and independent activity might be ever so good. The cases may yet be found where sufficiently narrow transverse lesions will allow the suturing of fresh-cut ends of the cord.

In other cases it remains yet to try the suturing of nerve-roots from above the breaks into some one or two below, sacrificing only the cutaneous supply for a small area, as suggested by Dr. Dana, or, as seems feasible, implanting them into the cut end of the lower stump, thus perhaps innervating some limited portion of the distal end with the chance of stimulating more. Possibly in this way even the vesical control alone might be restored. This is conjecture entirely, and is based on the fact that clean-cut nerve-sections will unite" (Robert Abbé, *New York Medical Record*, vol. xxxviii., 2, pp. 85–92).

Dandridge remarks that "the question of operative interference in spinal surgery must be considered in reference to interference immediately after the receipt of injury, and interference deferred until the amount and character of permanent disability can be determined. The solution of these questions involves a consideration of the source and results of fracture treated without recourse to operation. It is well known that not only a considerable number of these cases escape death, but entirely recover all their functions, even after the existence of paralysis, incontinence, and bed-sores existing for a variable length of time. In a valuable paper Burrell has investigated the results of all the cases which have occurred in the Boston City Hospital, eighty-one cases in all, of which eighteen survived. Divided into regions, we find twenty-eight cases of fracture of the cervical vertebræ gave two recoveries; twelve cases of fracture of the upper dorsal vertebræ, six recoveries; nineteen cases of fracture of the lower dorsal, one recovery; twenty-three cases of fracture of the lumbar vertebræ, ten recoveries. Of the sixty-four fatal cases thirty-five died within five days, eight in from five to ten days, and seven in from ten days to a month. Five of these cases were submitted to operation,—all of which were promptly fatal. In the eighteen who survived, the result was good in nine, and in nine complete disability remained permanent. The especial value of Burrell's paper is that it is based on the entire number of cases treated in a single institution, and gives, therefore, more reliable data for deductions than statistics made up of isolated cases. Favorable results always obtain undue prominence under such circumstances. It is apparent from the above figures that twenty-one per cent. of fractures of the spine survive, and that over ten per cent. make a satisfactory recovery.

"Of the fatal cases, a very large proportion die in the first few days,—a fact of importance in estimating the danger of operation, the high mortality of which in cases of recent injury cannot properly be attributed to the operation itself, but is largely due to the concomitant injury.

"It is further evident that, while treatment is successful in preserving the lives of a considerable number, one-half the survivors are left completely disabled: so that any addition to our therapeutic resources must be eagerly welcomed" (*Journal of the American Medical Association*, 1889, vol. xiii., 2, pp. 39, 40).

teum reflected on the opposite side. Cline and Tyrrell used broad angular plates as retractors on each side; Gordon divided the attachment of the muscles to the articular processes, which, he said, gave plenty of room, as each end of a muscular bundle when separated from its attachment retracted of itself and needed little holding back with the fingers; Horsley divides the deep fascia at right angles to the line of the incision opposite its middle, in order to prevent its resisting a proper separation of the sides of the wound, and says that it may be found necessary to divide this aponeurosis at more places than one. In my cases I found that retractors of moderate size, not large or clumsy enough to be in the way, were all that was needed to keep the muscular masses from interfering with the subsequent procedures. The next step, and one which greatly facilitates the remainder of the operation, consists in the division of the spinous processes close to their bases by means of large, strong bone-forceps set at an obtuse angle. That was not done by most of the earlier operators, but it certainly adds nothing to the severity of the operation, while affording much freer exposure of the laminæ, which are the next parts to be attacked. In perforating these, Cline advised the use of a small crowned trephine to cut through the vertebral arches, if requisite,—that is, if they were not already broken into more than two pieces. Tyrrell found Hey's saw most convenient. In Gordon's case one side of the arch was broken and the yellow ligament torn through, admitting the entrance of his finger, so that he merely divided the other side of the arch with a pair of stout bone-forceps; Lauenstein employed a chisel; Horsley returned to the use of the trephine, but says that if more than one arch is to be removed it will be better by means of an angular saw to cut partly through the laminæ along the lines of the sides of the neural canal, and then the division of the bones can be completed with the bone-forceps. I have found that both the trephine and the saw are unnecessary, and that the laminæ can be more expeditiously, and with equal safety, divided by the use of a pair of very strong bone-forceps, either straight or having a large obtuse angle, as may be preferred by the operator. The vertebra at the centre of the incision should be selected, and the vertebral spaces above and below its laminæ should be recognized with the tip of the finger. The points of the bone-forceps should then be placed above and below its edges, and the laminæ cut through by successive short nips, the line of section being as close to the transverse processes as possible. This gives the greatest exposure of the cord and of the membrane itself.

Abbé's procedure is as follows. The patient is laid prone, but with one shoulder raised by a sand pillow,—favoring easier respiration and inclining the back towards the operator slightly. A free incision is made parallel to the spines and a half-inch to one side, cutting the longitudinal attachments from one side only, and being carried clean down to the laminæ at the second or third pass of the knife. To approach the fracture the incision is made from one or two vertebræ above to an equal distance below; the laminæ are then cleared of muscles, which are drawn outward by retractors, and the ligament divided so as to isolate a block of four spines, whose bases are then severed from their arches by stout cutting pliers.

This manœuvre at once allows a retraction of the entire block of connected spines with their muscles still attached to one side, and the entire breadth of the spinal arch is thus exposed without sacrificing the overlying tissues. A pair of slightly-curved rongeurs are then applied to the lower edge of one lamina, and with incredible ease the entire breadth is quickly gnawed away.

Abbé claims that this is the most speedy, the least bloody, preserves all the tissues in and about the spines which are replaced, and gives firmness to the back, as well as prevents a gap that nature would be obliged to fill. The incision should be on one side of the spines only, they being cut off and dragged to the other side, exposing the entire arch without dividing the interspinous ligament. The only bone-cutting instruments to be used are two very narrow rongeurs, one curved, one straight, with a quarter-inch bite and presenting a flat Gothic arch aspect when the points are brought together. Abbé thinks that the saw is dangerous, awkward, and inefficient, and that the rongeurs are by far the quickest.

The color of the dura should be noted, particularly with the view of determining in cases of traumatism whether it is or is not necessary to open it. If it be dark or purplish from the presence of exuded blood beneath it, or yellowish from the presence of pus, it will, of course, be proper to incise it in order to empty and to explore the subdural space. My own feeling is that in all cases in the future in which this operation is performed for supposed tumor, and the growth is not found lying upon or external to the dura, that membrane should be freely and fearlessly opened for exploration purposes. The resulting escape of the cerebro-spinal fluid has been found to be merely one of the bugbears of the subject, and is certainly not to be weighed against the chance of terminating the operation without discovering a possibly existent and relievable condition. If it is determined to open it, it may be picked up in the median line and at the middle of the incision with a pair of delicate toothed forceps, nicked with a knife or the scissors, and then divided either upon a director or with a pair of blunt-pointed scissors to any required extent upward or downward. It can be easily and gently retracted to either side, so as to expose the whole posterior surface of the cord to both inspection and palpation, permitting the gentle insertion of the tip of the finger between its inner surface and the lateral aspect of the cord, and permitting also, if need be, the investigation of the anterior and antero-lateral subdural spaces by means of a blunt curved instrument, such as a pedicle needle or an aneurismal needle. The inspection having been completed, the tumor having been removed, and the pus or blood evacuated, or it having been determined that no indication for further operative interference exists, the incision in the dura should be stitched up with fine interrupted catgut sutures. These may be introduced very readily and speedily by means of a pair of staphylorrhaphy needles, the one not in use being threaded by an assistant while each stitch is being put in place; the stitches should be inserted at intervals of about one-eighth to one-sixth of an inch, and when all are in place can be easily tied, the ends being cut off short. A small rubber drainage-tube and a dozen strands of chromicized catgut should then be laid throughout the entire length of the wound, the muscles being united above them by means of chromicized catgut sutures, after which the skin and aponeurosis are brought together by silk or silver wire, as may be preferred. It is hardly necessary to say that the most rigid antiseptic precautions should be observed from first to last.

I may, in conclusion, briefly summarize the opinions I have arrived at after this consideration of this last branch of the subject.

1. Some objections urged against operative interference in spinal traumatisms—*i.e.*, hemorrhage, frequency of absolute destruction of the cord, pressure from inaccessible fragments of bone, etc.—have been shown to be unsupported by clinical facts; others were largely due to a well-founded dread of (*a*) the shock in those cases operated on in pre-anæsthetic times, and (*b*) consecutive inflammation, suppuration, and pyæmia in pre-antiseptic periods.

2. The results of recent operative interference in properly-selected cases of fractures of the spine are encouraging, and should lead to the more frequent employment of resection of the posterior arches and laminæ: (*a*) in all cases in which depression of those portions, either from fracture or from dislocation, is obvious; (*b*) in some cases in which after fracture rapidly progressive degenerative changes manifest themselves; (*c*) in all cases in which there is compression of the cauda equina from any cause, whether from anterior or posterior fracture or from cicatricial tissue; (*d*) in the presence of characteristic symptoms of spinal hemorrhage, intra- or extra-medullary.

3. Operation is contraindicated by a history of such severe crushing force as would be likely to cause disorganization of the cord. The question which will remain in doubt previous to operation will usually be that of the extent of damage done to the cord and the possibility of its taking on reparative action. As to this, the safest rule is that which has been formulated by Lauenstein,—namely, that if after the lapse of six or ten weeks there is incontinence of urine with cystitis, or incontinence of fæces, and especially if there are also the development and spreading of bedsores, but little is to be hoped for from the unaided efforts of nature. If, however, these symptoms be absent, and if there be the least improvement in either sensation or motion, it will be proper for the surgeon to delay operative interference still longer.

PRELIMINARY NOTE ON THE PHARMACOLOGY OF SALICYLAMIDE.

By W. B. Nesbitt, M.D., Toronto, Canada.

THIS compound is one of a series which I have been experimenting with in a study of the modification of physiological action through chemical constitution, in this work the action of the amidogen radical NH_2 being chiefly considered.

In this connection I might mention that a few years ago I made some notes on the introduction of the amidogen radical into chloral,* as it occurred to me from my experiments that this combination might be of benefit. Although it has not been therapeutically tested, yet Dr. Langaard, in an article† on the "Newer Hypnotics," says that the great majority of these—chloral-formamide, chloralamide, chloral-urethan, etc.—are based upon the principle of introducing the stimulating amidogen radical into chloral, mentioning as the originator of this method chloral-ammonium.‡ So that I feel that if a tenth of the claims made for chloralamide are true my work has not been in vain.

As stated of chloral-ammonium, so of salicylamide, I give these notes with the belief that this compound may be of value for two or three reasons:

1. It is a modification of a long and favorably known remedy,—salicylic acid.

2. It is as tasteless as the latter is unpleasant.

3. If, as stated by Schmiedeberg, Brunton, and numbers of other pharmacologists, there is an especial stimulating action in the amidogen radical, it should be safer than salicylic acid.

It is the amide of salicylic acid, with the formula

$$C_6H_4{<}^{OH}_{CONH_2}.$$

First prepared by Limpricht, it is described by him§ as made by the action of concentrated ammonia on methyl-salicylate. It crystallizes from ether in lustrous yellow plates, and sublimes when carefully heated. As a phenol, it forms metallic salts; its ethers are formed when ammonia is allowed to act upon the ethers of salicylic acid.‖

I have prepared it by the action of concentrated ammonia on oleum gaultheria, which is chiefly composed of methyl-salicylate. This method, although causing greater difficulty in separating out a pure salt, as a large quantity of very soluble brown coloring-mat-

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* Therapeutic Gazette, February, 1888.

† *Therapeutische Monatshefte*, October and November, 1889.

‡ Therapeutic Gazette, *ibid.*

§ "Annalen der Chemie und Pharmacie," vol. xcviii. p. 258.

‖ Roscoe and Schorlemmer, vol. iii., Part IV., p. 312.

ter is formed, has the advantage that no toxic substances are formed, as has been the case in the manufacture of artificial salicylic acid.*

I have also prepared it by heating ammonium-salicylate. This, although a common method for the preparation of the amides of the fatty series, has not been used for salicylic acid, and I mention it chiefly to show the difference between ammonium-salicylate and salicylamide.

When ammonia and oleum gaultheria have stood together in the cold for some days, the liquid assumes a deep reddish-brown color, and brownish crystals begin to separate; all impurities are, however, readily removed by a few recrystallizations.

The method of crystallization depends upon the rapidity of cooling and saturation of solution. If rapidly cooled from a *saturated* solution, they appear in fine acicular tufts. The usual method of crystallization, however, is either in thin long plates or bunches of these. It is easy to procure by means of charcoal perfect colorless crystals and in thin transparent plates; melting-point, 142° C.; soluble in alcohol, ether, chloroform, and water; about twice as soluble in cold water as salicylic acid (1 in 250), but readily soluble in hot water. It is quite tasteless, and gives a somewhat gritty sensation between the teeth. Its chemical relations are readily shown by a comparison of the structural formulæ:

$C_6H_4<^{OH}_{COOH}$ Salicylic acid.

$C_6H_4<^{OH}_{CO,NH_2}$ Salicylamide.

$C_6H_4<^{OH}_{COOHNH_3}$ Ammonium-salicylate.

After administration its presence is speedily recognized in the urine by the violet coloration on addition of ferric chloride, following the white precipitate of phosphate of iron which is first produced. It is excreted to a slight extent unaltered, but chiefly as salicyluric acid, or, more correctly, hydrocyanic hippuric acid. In this respect it differs from amides of the fatty series; for instance, acetamide, which is excreted unchanged.† It has a decided effect upon the urine, in that it prevents putrefaction for months.

Passing from the more purely chemical side to the physiological, I find that both the diastatic and peptic changes are retarded by it, but not to the same extent as by salicylic acid. It lowers the coagulating point of albumen by five per cent., and from this we would expect that oxidation would be diminished, but testing protoplasm with guaiac in its presence it gives very slight retardation. This may be due to the fact that, although the coagulating point is lowered, it is not to any great extent.

As mentioned above in connection with the urine, it shows decided germicidal properties. On testing this by inoculating Cohn's fluid, in which salicylamide was dissolved, with micrococci and bacteria, it was found that a strength of 1 in 500 prevented all development, while 1 in 750, though preventing development of bacteria, did not prevent development of micrococci.

In very dilute solutions it paralyzes the amœba, and eventually kills them, though, if washed with fresh water just after paralysis takes place, movement recommences. This is likewise true of its action on the leucocytes of human blood.

Brunton states‡ that, as a general rule, most of the compound radicals formed by the union of amidogen with the radicals of the marsh-gas series possess a paralyzing action on motor nerves. I think this may be stated with equal truth of the union of amidogen with members of the aromatic series. On administering salicylamide *per os* in toxic amounts to frogs there is a uniformity of symptoms. First, a period of decided but temporary excitement, with slight quickening of respiration and engorgement of superficial veins: concomitant with this there is an approaching general failure of movement, which first appears in the lower extremities, the frog still keeping up active movements of the fore legs, but this shortly ceases and the frog gradually dies. If the chest-wall is opened and the heart examined, it will be found to be beating very rapidly with perfect rhythm, but towards death it slows down and rhythm ceases, the auricles beating often as many as 8 or 10 beats to 1 of the ventricles, and the heart finally ceases in great diastole, engorged with venous blood; in fact, for some time before death, this venosity of the blood is characteristic.

On examination of its action on the different structures of nerve, muscle, cord, etc., the following may be observed:

* "Artificial Salicylic Acid," in *British Medical Journal*, 1890.

† O. Schutzen and Mencki, *Virchow's Archiv*, Band 58.

‡ Brunton's "Materia Medica and Therapeutics," p. 52.

1. *Action on Nerves.*—In the frog poisoned by salicylamide, when loss of movement ensues, a blow on the extended leg produces no effect, but a pinch after a short delay (the delay depending upon extent of poisoning) will cause the frog to draw up both legs. It thus seems to have an action similar to that of many analgesics, the irritation causing a diffusion of motor impulse in the cord.*

On performing Türck's experiment † for determination of reflex action, the result is found to be completely confirmatory of the above, so that there is no central effect. In Türck's experiment the diffusion of motor impulses is likewise shown, as on irritating the toe with the acid, after a short interval, both legs are drawn up at the same time. From this it was necessary to test the action on the exposed nerve, the sciatic being used. Here the salicylamide was found to destroy the power of the nerve impressed in it long before the other one.

The next point to ascertain was whether there was any change in nerve-conductivity. To do this the sciatic was exposed, and under it was placed a strip of rubber, which held some salicylamide on the middle of the exposed portion of the nerve. The nerve was now irritated by a current above and below the point of application of the salicylamide, both giving muscular contraction. Irritation was made every two and a half minutes, the result being that in twelve and a half minutes irritation above had no effect, while irritation below gave reaction for more than two hours.

The supposition that it is motor-conduction in the cord that is interfered with, as stated before, is further borne out by the fact that when the animal is poisoned, the effect is seen first in the lower extremities, motion and power to move itself along persisting in the forelegs for some time after.‡ That the drug likewise has a certain effect on the conduction of painful impressions through the gray matter of the cord is evident from the fact that impressions which are ordinarily painful, as a blow, produced no reaction, while tactile impressions still produce reflex action.§ On muscle it was found to destroy the irritability, but not nearly so rapidly as in the nerves; for instance, a nerve-muscle preparation placed in a salicylamide solution (1 in 250), the reaction of muscle through the nerve was lost in ten minutes, whereas the muscle would contract on direct stimulation for four hours afterwards.

The effect of the salicylamide on the tetanic curve produced by the faradic current was very striking, as a fifteen-minute immersion was sufficient to almost abolish it.

2. *Action on the Heart.*—Having discerned its action on nerve and muscle separately, we are in a position to consider its effects on the heart. A few preliminary experiments showed that its general effect was first stimulation, and with greater rapidity and more complete systole and diastole, and then gradual slowing, with final stoppage in diastole. This latter effect may have been due to irritation of the inhibitory apparatus or paralysis of the motor apparatus.

Experiments made to determine the effect of administering the drug *per os* on the heart show that slowing still went on, despite the application of nicotine and atropine, showing that there was no irritation of central or inhibitory apparatus.

That the inhibitory apparatus was not interfered with was further determined by direct experiment.

The following experiments give a very complete picture of the process of exclusion in locating the action of the poison.

EXPERIMENT 1.

Time. h. m. s.	Pulse per 30 secs.	Remarks.
2.55.00	...	Pithed rana pipiens.
3.00.00	19	
3.05.00	19	
3.30.00	...	Vagi exposed.
3.33.00	19	Cold saturated solution of salicylamide.
3.34.00	18	
3.35.00	17	
3.36.00	17	
3.38.00	17	
3.40.00	14	Pulsation weaker.
3.42.30	14	
3.45.00	14	
3.46.00	...	Cold saturated solution of salicylamide.
3.49.30	13	As a rule the drug gradually diminishes the strength of pulsation concomitantly with the slowing. On irritating the vagus trunk the heart was stopped; therefore the inhibitory apparatus in the heart is not destroyed.
3.50.00	13	
3.52.30	13	
3.55.00	13	
4.00.00	13	

* Brunton's "Croonian Lectures" (*British Medical Journal*, 1889).

† R. Meade Smith "Experimental Pharmacology."

‡ Hughes Bennett, *Edinburgh Medical Journal*, October, 1873.

§ Brunton's "Materia Medica and Therapeutics," p. 792.

EXPERIMENT 2.

Time. h. m. s.	Pulse per 30 secs.	Remarks.
7.26.00	16	Administered 5 grains of salicylamide *per os* in a rana pipiens on opening chest-wall. Heart's action good, although extensive and almost complete loss of motion; sensation on pinching, slight response, but if pinched immediately afterwards, no response, as if the motor discharge sufficient for contraction was exhausted after one effort; sensation is also delayed.
7.30.00	14	
7.35.00	14	
7.45.00	12	
7.50.00	10	No attention to any stimulus, except conjunctival irritation.
7.52.30	9	
8.15.00	7	
8.20.00	8	Applied drop of nicotine solution to central control.
8.35.00	7	
8.40.00	7	$\frac{1}{100}$ gramme of atropine.
9.00.00	6	No inhibitory action.
9.15.00	...	Heart excised and placed in NaCl. solution.
9.16.00	9	
9.21.00	9	Placed in salicylamide solution the heart made 7 beats, and then stopped.

EXPERIMENT 3.

Time. h. m. s.	Pulse per 30 secs.	Remarks.
4.50.00	...	Exposed heart and vagi in a rana pipiens.
5.00.00	20	
5.01.00	21	
5.05.00	20	
5.08.00	20	
5.09.00	...	Administered salicylamide.
5.10.00	18	
5.12.30	16	Pulsation weaker.
5.15.00	16	
5.17.30	15	Pause longer.
5.20.00	15	
5.22.30	14½	Vagi divided.
5.25.00	14	No central control.
5.27.30	14	No central control.
5.30.00	14	Gave nicotine.
5.32.30	10	
5.35.00	11	Not due to irritation of vagi roots in heart.
5.37.30	11	Not due to irritation of vagi roots in heart.
5.40.00	10	$\frac{1}{100}$ grain of atropine.
5.42.30	9	Not due to irritation of inhibitory apparatus.
5.45.00	8	Not due to irritation of inhibitory apparatus.
5.47.30	8	Pause much lengthened; rhythm good.
5.50.00	7	
5.52.30	7	Now washed out pericardial sac with salt solution; beats rose to 10 immediately for 10 minutes, then salicylamide was applied, and the heart stopped in 20 minutes.

As there was never any acceleration of the pulse in the excised heart, the accelerating apparatus need not be considered, and from the foregoing experiments the inhibitory apparatus has been completely excluded, so that there is only the motor centre and muscle-substance to deal with. This is undoubtedly the most difficult point in cardiac pharmacology, as we have to do largely with hypothesis.

In this case we find that in nerve and muscle experiments both are paralyzed, but the nerve long before the muscle. We likewise find that where the heart has stopped beating it can by electrical or mechanical stimulation be made to give a few more beats, eventually dying in systole. This would not be the case if we had complete loss of function in muscle as well as nerve, or a loss of function in muscle preceding the nerve paralysis. So that it may be safely stated that death is due primarily to action on the motor apparatus.

Approaching the still more difficult ground as to whether this action is on motor-discharge or motor-conduction, I have given preference to motor conduction, for the reasons that,—

1. In the experiments on the exposed sciatic conduction was absolutely abolished.

2. In nearly all experiments on the heart, the mode of dying has been 1 beat of auricle to 1 of ventricle, then 2 of auricle to 1 of ventricle, 3 of auricle to 1 of ventricle, etc., much in the same manner as in Gaskell's experiments with the clamp, where there was direct mechanical interference with conduction.

3. *Action on Respiration.*—The action on respiration in the frog is only of negative value, as death did not occur with convulsions. It was necessary to administer to mammals, because if in them death occurred by convulsions, it would be due to the action on the respiratory centre.

To test this, the drug was administered to a couple of chickens. In them it produced first an ataxic gait, then a condition of hebetude, and finally death, without apparent convulsions. There was also no darkening of the comb, which is so readily noticed where any serious blood-change is produced. As regards its further action on respiration and

blood in mammals, I have not yet made a sufficient number of experiments to speak authoritatively on more than one point,—that is, where it was given to two rabbits, in the proportion per kilo in which it gave decided symptoms in man, the kymographic tracing was not affected by it.

Of its action on temperature in health, in two experiments on myself, doses of 3 grains, with fifteen minutes intervals, nine grains in all, the temperature was lowered in one instance 1⅘° F., in another 2⅕° F., accompanied with ringing in the ears and decided chilliness. A sphygmographic tracing, taken at the same time at the same pressure (three ounces), before and after salicylamide, shows a dulling of the apex of the first stroke. It is generally conceded that no estimate of blood-pressure can be made by the sphygmograph; yet, as the same pressure was used in both instances, this would seem to indicate a lowered vaso-motor action, with reduction of temperature, due to capillary dilatation or increased heat-dissipation. This, however, I will endeavor to more accurately determine by subsequent calorimetrical experiments.

The following therapeutic notes may be of interest:

CASE I.—Mrs. M., suffering intense pain in right ovarian region, probably due to congestion, took 5 grains every three hours, and obtained relief after third dose, fifteen grains in all, being the largest quantity I have yet administered in that time. She complained of slight rash around waist and on one side of face, where she had been sitting near a fire; no other ill effects noticeable.

CASE II.—J. D., aged 11; follicular amygdalitis; rheumatic history; pain was relieved by 3 grains of salicylamide; the throat was clear on the following morning.

(I have notes of four similar cases.)

CASE III.—Park S., suffering from severe headache: temporal and facial arteries contracted; relieved by 3-grain doses every hour.

CASE IV.—Mrs. K., chronic rheumatism; attack of pain relieved by 5-grain dose; no depressing effect, although there was a cardiac complication present.

To sum up these results.

Pharmacologically it prevents conduction in nerve; paralyzes nerve first, then muscle.

On the heart, its chief effect being on motor apparatus, most probably through its activity on conduction.

Diminishes spinal reflex for motor impulses.

Diminishes spinal conductivity for painful impressions.

Diminishes muscular irritability.

In mammals no particular effect on respiration.

No particular effect on blood-pressure. In medicinal doses reduces temperature, causes ataxic gait, and hebetude in the fowls.

Therapeutically its advantages over salicylic acid and its salts are,—

1. It has no taste.
2. More soluble than salicylic acid.
3. It acts more promptly.
4. It acts in smaller doses.
5. It has greater analgesic properties.
6. It is pharmacologically safer.

In conclusion, my most sincere thanks are due to my former teacher, Professor W. H. Pike, for the kind manner in which, on this and previous occasions, he has allowed me the use of the chemical laboratory in University College, and also in the present instance to Dr. Charles Sheard for the loan of his physiological apparatus from the laboratory of Trinity Medical College. I feel great pleasure in thus being publicly able to thank these gentlemen, as they have thus done much to help me over the rough road of pursuing investigations, as we so often have to do, at the expense of our own pocket as well as our time.

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*THE LIMITATIONS OF BACTERIOLOGICAL THERAPEUTICS, WITH SPECIAL REFERENCE TO TUBERCULOSIS OF THE LUNGS.**

BY E. P. HURD, M.D., NEWBURYPORT, MASS.

I SHALL discuss my subject under three heads:

1. What is meant by bacteriological therapeutics?
2. The limitation of bacteriological therapeutics in infectious diseases in general.
3. The limitation of bacteriological therapeutics in pulmonary tuberculosis.

1. BACTERIOLOGICAL THERAPEUTICS PRIMARILY ANTAGONIZE BACTERIA.

There is now a general consensus of belief in the bacterial origin of all infectious diseases. That all communicable maladies, such as can be imparted from plant to plant, from animal to animal, from the animal to the human being, from one man to another, are due to

* Read before the Climatological Association, Washington, D. C., September 23, 1891.

micro-organisms, in such a sense that without the latter the diseases would be non-existent, is not now a matter for debate; there are sufficient scientific facts to prove the proposition in its application to many of them, and, from what we know of the uniformity of natural laws, the inference is irresistible that all are microbic diseases.

Among the bacterial diseases of the lower animals whose causal agents are known, we may enumerate fowl cholera, the contagious acne of horses, pigeon-pox, American swine plague, European *rouget*, glanders, and charbon. Of diseases peculiar to man, the infectious agents of the following have, with sufficient certainty, been determined: typhoid fever, diphtheria, cholera, tetanus, pneumonia, erysipelas; and I believe that we are warranted in adding malaria, though the latter is not due to bacteria, but to infusoria.

The problem of tuberculosis has not been worked out in its entirety; there are still difficulties and contradictions which are hard to explain in accordance with the new etiology, yet on the whole the causal relation of Koch's bacillus to tubercle seems to have been sufficiently demonstrated.

The infinite variety and multitude of micro-organisms; their *rôle* in the phenomena of fermentation and putrefaction, and in the general break-up of organic life; the incessant struggle of the cells of the living organism against the microbes, and especially against such as are pathogenic, are subjects on which it is not necessary for me to dwell. What is particularly worthy of note is,—1. Their extraordinary reproductive activity, and the possibility, under favoring conditions, of the anatomical elements being overcome and overwhelmed by them. 2. Certain pathogenic species penetrate the tissues, irritate and give rise to inflammations and morbid growths. 3. Where they find a congenial habitat, they, parasite-like, live at the expense of their host, appropriating oxygen and pabulum, and thus impoverishing the tissues. 4. They secrete or excrete certain poisonous principles,—ptomaïnes, toxines, tox-albumens,—to which the baneful effects of many of them are due.

The habitat of the microbes may be all or any of the tissues and fluids, but ordinarily each pathogenic species has some favorite localization (lungs, intestines, etc.),—some *locus minoris resistentiæ* where it best thrives and multiplies.

Bacteriological therapeutics comprise such medicinal and hygienic means as directly antagonize the bacteria that cause the disease. The term is nearly synonymous with the term *antiseptics*, and is applicable to everything which is prejudicial to the life, multiplication, and functions of the microbe, in doses which are safe for a man to take.* This definition is, as Bouchard points out, a purely medical definition. The surgeon, acting only on limited surfaces, may sacrifice certain parts of the body attacked by the infectious agents. When, however, we come to the practice of general antisepsis, the problem is of a different nature, and this brings me to my second heading.

2. THE LIMITATIONS OF BACTERIOLOGICAL THERAPEUTICS IN INFECTIOUS DISEASES IN GENERAL.

If the views just stated are sound, then an infectious disease may be defined as "a conflict between the subject who is smitten and a particular micro-organism which multiplies at his expense, appropriates his air, water, and pabulum, disintegrates his tissues, or poisons him by its secretions, or by the decompositions which accompany its development." † Granting this definition (to which daily increasing knowledge lends support), the study of bacteria in general, and especially of the pathogenic bacteria, possesses a surpassing interest to every physician who looks for enlightenment and help from the new pathology; for it is not too much to affirm that as the causes of diseases are understood, the triumph of therapeutics becomes more easy of attainment.

It has long been the reproach of the medical profession that therapeutics is not an exact science. With a full knowledge of pathogenic germs and germicides that are available and safe, physicians may eventually feel that the reproach of the ages is no longer justifiable, at least with respect to the microphytic diseases. The art of the physician in presence of these diseases will consist in the observance of a few simple rules: What is the nature of the particular infectious disease, and is the causal agent, the parasite, known? Then all the recognized agents of the parasiticide medication will be called into use to prevent the development and multiplication of the microbe. What can be accomplished by intelligent and watchful prophylaxis to stay the spread of the infection must first be done, and human habitations and persons protected by suitable antiseptic means. In fact, it is from prophylaxis that the first, and, thus far, the principal

* Bouchard. † Duclaux.

services have been realized, and sanitary science, surgery, and obstetrics have derived incalculable benefits from bacteriology, despite the fact that this science is yet in its infancy.

As for the unfortunate victims of microbic invasion, the problem of the therapeutist presents this difficulty: How best to destroy the parasite and not injure the patient? He may not be able to apply his germicides in quite as unstinted a manner to the living human organism as he would to infected barracks or emigrant ships; but this leading indication he will ever have in view,—namely, to render the human environment of the morbiferous microbes as uncomfortable as possible, and to weaken and limit their powers of growth and multiplication. In short, when once the cause is ascertained, there can be but one leading indication,—namely, to destroy the pathogenic agent, or attenuate as far as possible its influence on the economy. The resources of the physician will be enhanced by the practical discoveries of the chemist and microbiologist; and experimental physiology will effect for all rival systems of medication "provings" which cannot be gainsaid, because of their strictly objective character.

Bacteriological therapeutics comprehends such chemical substances as are offensive and destructive to microbes in general, and certain pathogenic microbes in particular. Possibilities in this direction are limited by the ability of the living cells of the organism to tolerate the bactericide agent. This is, and always must be, the principal limitation. It is true that much hope is expressed by some enthusiasts that just as $\frac{1}{150,000}$ of nitrate of silver will destroy a culture of *aspergillus niger*, so for each pathogenic bacterium some specific, harmless or nearly so to the animal cells, while fatal to the parasite, will yet be discovered. I am inclined to believe that such hope will always be like the "purification of our politics," "an iridescent dream."* Nor do we yet know enough about the *modus operandi* and the real value of the immunity obtained by "protective inoculations" (whether of the microbes themselves or the chemical products elaborated by them) to pronounce definitely as to the future of this mode of treatment. I have lately found the farmers of Nebraska full of faith in Dr. Frank Billings's protective inoculations against hog-cholera. Billings inoculates healthy animals with a pure culture of the very microbes that induce the disease, only selecting mild cases from which to obtain the material for his cultures. I have myself (it seems to me) witnessed the triumph of this method of protection.

On the other hand, Pasteur's anthracoid vaccinations have not been a complete success, and always there seems to be an unknown factor of danger, as has repeatedly been illustrated in the mortality which has attended vaccinations against *rouget* and *charbon* in different parts of Europe.

It is not necessary that I should do more than remind this audience of the sanguine expectations and enthusiasm which were lately awakened by Koch's tuberculin, and the miserable disappointment which followed. Nevertheless, I believe that these "bactericide toxines" and "defensive proteids" are destined to play a large part in the therapeutics of the future. They, in fact, come very near to the realization of what therapeutists are looking for, being substances which, elaborated by the microbes, or, as some say, the animal cells, are destructive to the microbes while being inoffensive to the cells of the organism. So that, possibly, our progress in this department of bacteriological therapeutics is limited only by our ignorance.

3. THE LIMITATIONS OF BACTERIOLOGICAL THERAPEUTICS IN RESPECT TO TUBERCULOSIS OF THE LUNGS.

The limitations of bacteriological therapeutics are often due to the fact that the culture soil—the environment, not the microbe—is the principal factor. This is especially the case with tuberculosis. And this naturally introduces the third topic of this paper.

For a time subsequent to Koch's discovery of the bacillary origin of tuberculosis, the special attention of experimental therapeutists was directed to the various antiseptic methods which, one after the other, were vaunted as efficacious against the bacillus. Inhalations took a prominent place: sulphurous acid, creosote, chloroform, carbolic acid, compressed air, ethyl iodide, terebene, eucalyptus, turpentine, benzoin, hydrofluoric acid have all had their advocates and their day. Bergeon's gaseous rectal injections of hydrogen sulphide were given with the same intent; it was believed that the gas which was introduced by the rectum would be eliminated by the pulmonary mucosa, favorably modifying the latter against the bacilli, if not actually destroying the germs *in situ*.

It must be acknowledged that the results of

* Ex-Senator Ingalls.

germicide treatment (and under this title must be included the hot-air-respiration cure of phthisis) have not been what was hoped. As patients sometimes temporarily improve under any kind of treatment, so some consumptives did get better after subjection to the fumes of burning sulphur and after the fluorhydric inhalations; the success of this medication was not, however, sufficiently satisfactory. Certainly laboratory experiments, such as those of Jaccoud, Grancher, and Chantard, have proved that the action of the vapors of fluorhydric acid on the evolution of experimental tuberculosis is *nil*.

The same may, I believe, be said of sulphurous acid vapors in such dilution as can be tolerated.

It is needless to say that there is no medicament yet known which, introduced by the mouth or subcutaneously, can by any direct bactericide power in the least arrest the development of either experimental or spontaneous tuberculosis.

Nor can we hope for much good from the introduction of any microbicide toxine, such as may be found to oppose the development of cultures of the bacillus tuberculosis *in vitro* or arrest the progress of experimental tuberculosis. Phthisis is too predominantly a question of soil (*terrain*) rather than of a specific bacillus. Unlike syphilis, the virus of which is capable of undergoing evolution in any soil, the micro-organism of tuberculosis does not attack all persons, it selects its subjects. "The individual who finds himself in a fairly normal condition of organic equilibrium resists with success, and burns the bacillus when the latter penetrates his air-passages. For this reason tuberculosis is the least contagious of all the microbe diseases. In short, the question of soil is primordial and fundamental; it occupies the first place in prophylaxis and treatment."* And no therapeutist can do good work in combating phthisis who does not start from this datum.

Some (as Cazeaux and Dujardin-Beaumetz) have advised that on all suitable occasions the windows of sleeping-rooms should be left open at night. It has been remarked that a patient will tolerate in a horizontal position, and consequently in bed, temperatures which seem severe when one is sitting or standing. At the same time, physicians cannot be too much cautioned against sending to elevated stations patients with advanced lesions and with fever.

The medical treatment, while not to be neglected, is only of secondary importance; nevertheless, under the use of the classic remedies—cod-liver oil, malt extracts, arsenic, creosote, tannin, hypophosphites, alcohol, cinchona, and nux vomica—one will often witness an augmentation of weight and favorable modifications in the local signs. As adjuvants, inhalations, now no longer relied on as curative, may produce some amelioration. In early phthisis there is much hope in climatotherapy, and this is unfortunately the principal hope.

Such are the principles of treatment which have given me what little success I have ever had, in a practice extending over more than a quarter of a century. Knowing that tuberculosis is simply a *blight* which smites imperfectly-nourished tissues, I would urge that the efforts of the therapeutist be directed to the element of cellular weakness rather than of chasing the will-o'-the-wisp of a bacillus. Evict one hungry brood, and another more voracious and more malignant will take its place.

If it should be argued that all these hygienic means and all medicinal means designed to fortify the organism and save it from the depredations of bacteria are properly *bacteriological*, the reply is that they are only indirectly, not primarily, so.

What would be the result of living in complete isolation from all bacteria (if that were possible) I cannot conjecture, so important are these "infinitely little" organisms in all the phenomena of life, death, and disease.

> "Partout la vie est dans la vie
> Et partout la vie dévore la vie."

Doubtless if the phthisically predisposed could pass their lives in altitudes never invaded by the specific bacillus, many of them might have immunity from tuberculous disease, for the same climate that is fatal to the microbes fortifies the lungs and the constitution. But what shall we say of those who have derived from parental blood the very germs of the disease which remain latent in the organism till the time shall have come when the pulmonary or other tissues, smitten from birth in their vitality, become fit spoil for the parasitic foe? That there are multitudes of such cases which, by the hard conditions of their being, are destined early to succumb; men and women that are weighed

* M. Cazeaux in *Bull. et Mem. de la Soc. de Méd. Pratique*, 1888. I do not know how it can be proved that the individual with well-resistant lungs *burns* the bacillus. It is more in accordance with the present status of science to say that the bacillus is devoured by phagocytes or asphyxiated in uncongenial blood-serum.

down by an hereditary curse which no fortifying environment can ever remove; that the inception of the disease means to them death; that no treatment—hygienic, bactericidal, medicinal—can do much even to ameliorate their condition; that such patients, whether belonging to the higher or lower classes, are and must for generations be the despair of the physician: these are facts of which we are all too painfully cognizant. But this is tantamount to affirming the limitations of all therapeutic means in grave tuberculous disease, owing to a fundamental, irremediable lack of vital resistance and vital endurance.

THE ACTION OF CHLORAL HYDRATE ON THE KIDNEYS.

Since the publication of Liebreich's monograph on chloral there has been no study of the action of this remedy on the kidneys sufficient to explain results which had been sometimes noted to follow the use of this valuable hypnotic. Dr. Cavazzini has, however, recently made some experiments in this connection on dogs and guinea-pigs, and he has found that when injected into the abdominal cavity, chloral hydrate produces marked irritation of the secreting cells of the kidneys. Even after the first injection it causes visible granular degeneration of the epithelium of the convoluted tubules, while after prolonged administration the epithelium of all renal tubules undergo degeneration, with the single exception of the straight tubes.

In more severe cases it produces swelling of all the renal epithelium, with other symptoms of acute parenchymatous nephritis, although the Malpighian glomeruli are never affected, nor is there ever any implication of the interstitial connective tissue.

The intensity of this destructive process depends partly upon the duration of the poisoning, partly also upon the individual's susceptibility. If the remedy is introduced through the stomach, it may likewise produce degenerative changes, but not to as marked a degree or as rapidly as when it is injected into the peritoneal cavity. The lesions thus described of the renal tissue are stated by the author to disappear after suspension of the use of this drug. Nevertheless the author states that in none of the cases in which the post-mortem examination proved this condition to be present, did he ever during life succeed in detecting any albuminuria, a fact which will perhaps explain the great scarcity of clinical observation pointing to the danger of chloral from its action on the kidneys. The author's observations, however, show that the kidneys are liable to marked disorganization from the action of chloral, and should serve to indicate the necessity for great caution in the employment of chloral in cases where the kidneys are already affected.—*Deutsche Medizinal-Zeitung*, September 7, 1891.

BRYONIA ALBA AS A REMEDY.

Dr. Huchard (*Rivista Clinica e Therapeutica*, 1891) calls attention to a very old remedy, which has during our own time escaped attention, namely, bryonia. Bryonia is a member of the Cucurbitaceæ. The part of the plant used is the root, which may attain the size of a man's arm, or even the thigh. The root is fleshy and yellow in color, while its juice is acrid and bitter. In the spring it is full of a white, irritating, and drastic juice. Its active principle is called bryonine. This remedy is one of the most ancient of medicines. Dioscorides praises its purgative and diuretic properties. Boerhaave used the dried root in wine as a cathartic in the treatment of dropsy. Harmand du Montgarny (1783) called it "the true European ipecacuanha," and used it in dysenteric diseases. Bryonia is a hydrogogue purgative which causes profuse watery stools, similar to those of jalap or senna. Too strong doses cause poisoning and a choleriform state. Huchard recommends the remedy as a purgative, 3 grammes (45 grains) of the powder at a dose, this dose producing in a case of hypertrophic cirrhosis with constipation five liquid stools and slight colicky pains; he also recommends it in whooping-cough, febrile diseases and inflammations of the respiratory tracts; in bronchitis, pleurisy, pneumonitis, etc. Bouchet used the tincture in febrile diseases. Dr. Louvet-Lamarre used bryonia in whooping-cough, where it diminishes tracheo-bronchitis, but does not shorten the duration of the disease. The dose is 1 gramme (15 grains) per day for a child of 7 years. During the spasmodic stage Lamarre prescribed the tincture of arosera, 1 gramme (15 drops) per day. Huchard uses a larger dose, 2 to 5 grammes (30 gtt. to 1¼ f3) of the tincture. The two remedies may be associated, 1 to 2 grammes of bryonia tincture to 2 to 5 grammes of arosera. The powder may be given ½ to 4 grammes per day.—*Cincinnati Lancet-Clinic*, September 19, 1891.

The Therapeutic Gazette

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Leading Articles.

THE TREATMENT OF GRANULAR CONJUNCTIVA.

GRANULAR ophthalmia is a disease which has existed from remote antiquity. There is, perhaps, no disease of the eyes more dreaded, both on account of its contagiousness and the gravity of the complications which it entails. There is no ophthalmic affection which has been the subject of so numerous and so varied kinds of treatment. According to Darier,* Hippocrates and his followers understood the importance of the local treatment of the granulations, for, besides the application of various collyria, they practised scraping with coarse cloth or pumice-stone. Cauterizations with the red-hot iron were also in vogue, and this method of treatment is in use among the Arabs today. Granular conjunctiva was the Egyptian ophthalmia from which the armies of Napoleon suffered so severely on their return to Europe, and from which so many thousands lost their eyesight.

Of all the innumerable caustics and astringents employed at different times, there are, says Darier, three principal agents which have proved their superiority,—nitrate of silver, as the best modifier of the conjunctival secretion; sulphate of copper, which may be considered as the specific of granulations; and acetate of lead, which is indicated in the slight forms, and in the forms of transition, which rightly or wrongly are called follicular conjunctivitis.† Of these, the sulphate of copper is the most universally used, and the safest. With the manner of applying the bluestone crayon to the everted lids the veriest novice in medicine is familiar. This treatment is not severely painful, and when carried out perseveringly every day for months does certainly cure a considerable percentage of cases. Després prefers the use of a collyrium of the sulphate of copper,‡ especially when the granulations are consecutive to a mild conjunctivitis. His formula is as follows: Sulphate of copper, 1 grain; distilled water, 1 ounce. Such a solution can hardly have aught but an astringent and temporary effect.

Is there a microbe peculiar to trachoma? Schongolowicz says there is, and this microbe is a short bacillus, best stained with gentian violet. He affirms that he has experimentally reproduced the disease in animals by inoculating a pure culture.§ All this lacks verification, as well as the discovery of the trachomococcus by Michel, though he claims experimental results of great interest; nor have Sattler's researches found confirmation. Certainly, however, the contagiousness of trachoma has been established by sufficient facts. It is hardly necessary to call attention to the fact that De Wecker's theory of the action of *jequirity* in this disease is that the latter develops a vegetable ferment on the diseased conjunctivæ, which antagonizes and kills the microbes of the disease, while exciting an inflammation which is destructive of the granulations.

In an interesting paragraph Darier describes the growth and propagation of the granulations, and compares this to the development of miliary tubercles or lupus. Around a mother granulation smaller ones are produced; but this phenomenon is even more apparent when the fibrous envelope is destroyed which covers the old granulations, and their contents are diffused over the con-

* *Bull. Gén. de Thér.*, t. ii., 1891, p. 74.

† *Loc. cit.*

‡ *Chirurgie Journalière*, p. 686.

§ *St. Petersburger Med. Wochenschrift* (cited in *Lancet*, August 23, 1890).

junctival surface or even over the cornea. Then the granulations are seen to spread with great rapidity.

Some caustics, as chromic acid, seem to stimulate the granulations to greater exuberance (this is also known to be the case with epithelioma and lupus), and hence it seems desirable that the treatment should be as radical as possible that complete destruction may be effected of the granulations and the infectious elements which they contain. Hence Peounoff, in the *Moniteur d'Optalmologie* (January and February, 1889), advocates the free use of the curette, which he prefers to any other procedure. He claims to have treated one hundred and eighty cases with success. Antisepsis is adhered to, and bichloride of mercury, 1 to 2000, is used (Maklakoff's "Report").

Professor Panas uses the galvano-cautery.* He first levels the granulations by the cautery laid on flat, and then he dips the needle deeply at different points, in order to reach the underlying strata of granulations.

Quaita has found corrosive sublimate almost a specific. He uses a 1 to 500 solution once daily, applied with a brush, and a 1 to 7000 solution several times a day, applied with cloths.† He finds this treatment well borne even with corneal complications.

Staderini speaks quite highly of this treatment by sublimate, and Luigi is also enthusiastic over it.

C. Bell Taylor believes the diplococcus described by Michel, and found by Schmidt and Sattler, to be the cause of trachoma. He quotes Power as saying,‡ "The diplococcus has its seat inside the trachoma-follicles but outside the cells, not on the surface but in the follicles of the conjunctiva; hence the method of squeezing, cutting, or scratching out the follicles is bacteriologically correct." Taylor thus describes his own treatment: "I scarify the lids, squeeze out the sago grains; occasionally, but not often, excise the retro-tarsal folds, and apply liquor potassæ, sulphate of copper, or syrup of tannin (nitrate of silver as a caustic does not go deep enough), and deal with bad cases by peritomy and inoculation with ophthalmia neonatorum or gonorrhœa (!), or infusion of paternoster bean."§ Cheatham advises "the use of jequirity, having found it unfailingly successful in his practice." Noyes‖ cautions against the use of too powerful caustics, which, indeed, destroy the granulations, but which leave bands of cicatricial tissue in the conjunctiva to act as mechanical irritants. Wicherkiewicz commends a powder composed of 1 part tannin and 3 parts finely triturated boric acid. In severe trachoma the galvano-cautery may precede the powder.

Darier¶ speaks highly of the treatment by corrosive sublimate, which, he says, is "infinitely less painful than the bluestone." He thinks that jequirity and inoculations with blennorrhagic pus have had their day. Of the curette he speaks highly, having found it successful in several cases. The lids are turned back and the granulations scarified with a cataract-needle, then, by means of a fine-cutting curette, all the morbid tissue is scraped off, care being taken to respect the conjunctiva. Anæsthesia by chloroform or ether is necessary to the comfort, if not complete success, of the operation. He now abandons the curette for a simple tooth-brush with short and hard hairs. An indispensable part of the operation is the scarification of the conjunctival envelope of the granulations, according to Sattler's method. By scarification the contents of the granulations are exposed, then, by means of the stiff tooth-brush above mentioned, all the morbid tissue is scraped away. When the tissue is sclerous, the cutting curette is necessary. The lids must be completely everted by means of forcipressure forceps, so as to expose all the granulations, which must be thoroughly scraped away, for, if any are left, the disease will again spread from this centre of infection. Afterwards the bleeding lids are carefully and energetically scrubbed with a solution of sublimate, 1 to 500. The dressing is then applied, which consists, for the first day, of compresses dipped in ice-water. The next day the culs-de-sac are cleared out by means of a probe with a little wadding wound around the point; the wadding is dipped in a sublimate solution, 1 to 500. The following days the lids are turned back as well as possible in order to clear them of the pseudo-membranous coat which lines them. Weak sublimate solutions are used throughout the treatment. Any granulations that may have escaped the brush or curette must be immediately snipped off by a pair of curved scissors.

* *Kansas City Medical Index*, 1888.

† "Sajous's Annual," 1888.

‡ Quoted from "Sajous's Annual."

§ *Bulletin de la Société Anatomique*, 1889.

‖ *Archives of Otology*, 1889 (quoted from "Sajous").

¶ *Bull. Gén de Thérapeutique*, July 30, 1891, p. 77.

The results claimed for this treatment are superior to any which this writer has observed under any other treatment. And whereas it formerly took many months and even years to cure inveterate cases, now, in the majority of cases, a few days suffice to effect a complete cure.

The number of patients successfully treated by Dr. Darier and his colleague, Dr. Abadie, by this method has been sixty.

THE THEORIES OF IMMUNITY.

DR. METCHNIKOFF, of the Pasteur Institute, at the recent meeting of the International Congress of Hygiene and Demography, restated and reaffirmed his views as to the bactericidal power of his phagocytes,—*i.e.*, the leucocytes of the blood. Since the phagocyte theory has been propounded, it has given rise to numerous objections, based on theoretical considerations, as well as on research and experiment; but the more we go into the question, the more it becomes evident that the army of the phagocyte cells is in reality a salutary power which is formed in the struggle of the animal organism for existence. At one time the phagocytes act alone, at another they combine with other factors which facilitate their efficiency. But in all these cases we see these cells combating the parasites. When the phagocytes are inactive, this is a sign either of the harmlessness of the microbe, or, on the contrary, of its extraordinary virulence. Dr. Metchnikoff instanced the case of remittent fever, in which it is known that the spirilla are destroyed by the phagocytes during the crisis and the period of apyrexia. In monkeys, where the spirilla are destroyed by the cells, the illness terminates with the first attack; but in man, where the phagocytes have generally not been able completely to destroy the spirilla, a second attack is produced, in spite of the formation of antitoxine. This is the same in intermittent fever. It is evident that it is only when the microbe is entirely destroyed that the organism can be considered free from it. Therefore, in this act of destruction of the microbes, it is the phagocytes which perform the most important *rôle*, as has been proved in the case of anthrax and in numerous other diseases, and in the case of the vibrio of septicæmia in guinea-pigs.

The question as to the bactericidal power of blood-serum was regarded as complicated and as needing fuller inquiry.

Behring, the founder of the blood-toxic theory, has himself acknowledged that this theory cannot be easily applied, except in diseases like tetanus and diphtheria, which are distinguished by their exceptionally toxic characters and by the absence of diffusion of microbes. As was proved by Vaillart and Vincent in regard to tetanus, the animals which are subject to this malady are naturally refractory to the microbe, and in this immunity from the microbe it is the phagocytes which play the principal part.

At the same meeting, Professor Emmerich read a paper on the "Artificial Production of Immunity against Croupous Pneumonia." He said that immunity was produced by antibacterial toxine being inoculated into the body. The blood of animals made immune possesses not only a very strong bactericidal power, but can be used as a remedy in infectious diseases when injected subcutaneously into the abdominal cavity or into the veins of a diseased animal, as had been proved in dysentery of pigs and in the croupous pneumonia caused by the diplococcus pneumoniæ of Fränkel. Considering that the experiments of Dr. Doenissen have shown that the diplococcus of Fränkel is the cause of the croupous pneumonia, we are entitled to assert that it is possible to cure men of pneumonia by the blood-serum of immunized animals, especially if we succeed in producing this liquid chemically pure and use it in proper quantities.

Dr. Adami said that the bacteriological world was at the present moment divided into two bitterly-opposed camps,—those who held that microbes within the system were destroyed by the agency of the living cells, and those who held that the function of microbic destruction was performed by the blood-serum and body fluids in general. He wished to especially emphasize the fact that we had ample evidence to show that the controversy upon the subject turns after all upon phenomena which differ not so much in kind as in degree. The controversy tended rather to be words than actualities, and if we directed our observations aright, it would be impossible not to be struck by the prevalence of phagocytosis. He was of opinion that those who were not in the thick of the fray must continually accept both views, and would point out that this position was far from being untenable. He said that he need hardly mention that it was especially around the phenomena observed in the rat that this controversy had raged, that in the rat phago-

cytosis could be with difficulty observed, and the rat's blood-serum possessed bacteria-killing properties to a high degree.

In the opening paper, read before the Bacteriological Section on the 12th ult., Dr. Roux, of the Pasteur Institute, said that the theory of immunity proposed by Metchnikoff does not deny that there may be other means of protecting the organism, but it affirms that the phagocytic action is of all these means the most efficacious and extensive. It seems to account for all the facts and to be eminently suggestive; it is thus that the knowledge of microbic poisons and chemical inoculation has thrown light on what would otherwise have been obscure. Far from being shaken by the theories that were opposed to it, this theory of Metchnikoff had gained by the opposition which it had met, and that was a guarantee of its genuineness.

Dr. E. H. Hankin stated his theory of immunity as follows: Immunity, whether natural or acquired, is due to the presence of substances which are formed by the metabolism of the animals rather than by that of the microbe, and which have the power of destroying either the microbe against which immunity is possessed, or the products on which their pathogenic action depends. It may be noted that this theory does not attempt to exclude other factors. He calls the substances on which the bactericidal action depends *defensive proteids*, but concedes that nothing is yet definitely known about them.

THE PHYSICIAN AS SURGEON.

PHYSICIANS practising in the country and general practitioners in large cities often complain of a dearth of surgical cases in their practice. In the country naturally grave accidents are of infrequent occurrence and occasions for major surgery are few. The complaint of the city practitioner is that the best surgical cases go to the hospitals for treatment, and if he is not on the hospital staff he is likely to fare hard for opportunities to exercise his talent and make for himself a reputation as a surgeon. Seeing little of surgery, he perhaps loses interest in that department of his profession, ceases to read in the medical journals the columns devoted especially to surgery, and eventually becomes awkward and unskilful in even the simpler operations in which he may be obliged occasionally to act as principal, such as tying the large arteries for dangerous hemorrhage, reducing a dislocated hip, putting up in splints a fractured thigh or humerus, excising a simple tumor, opening a deep abscess, or reducing a strangulated hernia. He has differentiated into a sort of diagnosticating and drug-prescribing machine, and has acquired a positive disrelish, if not horror, for the scalpel.

The above sketch is no libel on many practitioners otherwise excellent who may be found both in the city and in the country.

That it does not pay for the general practitioner to neglect surgical studies goes without saying. There is in a well-performed surgical operation a satisfaction which does not often attend the administration of powders and pills, and surgical specialists have little reason to complain that their practice is not sufficiently remunerative.

The patient for whom one prescribes may have an incurable malady, little amenable to medical treatment, so that the most careful and painstaking diagnosis and the most judicious advice, based on the leading "indications," may fail to confer much benefit. But the surgeon who removes a tumor that was compromising life, or saves his patient by a lithotomy or laparotomy, performs a service the material benefits of which are plain and indisputable.

How many physicians *medically* treat patients that demand surgical aid: neglecting to apply the curette to the diseased endometrium when months of suffering might thereby be saved, to exsect a necrosed bone which is causing great local and general disturbance, or to strengthen the incurvating spine of a rickety child by a Sayre jacket?

Opportunities for witnessing all kinds of surgery are sufficiently abundant in connection with the large hospitals, and the general practitioner will not fail to derive benefit from frequent attendance on these operations and on the surgical clinics. If this takes him now and then a day from his patients, he will certainly obtain hints that will be of value to him in his private practice. He must, in order to be truly successful, take all medicine for his province (even if he seems now and then to trench on the domain of the specialist), and show as much skill with the scalpel, or in adjusting a splint, as in diagnosticating a spinal sclerosis, or in laying down rules for the administration of mercurials in a syphilitic dermatosis.

PRESCRIPTION-WRITING.

THE day for complicated prescriptions seems to be over. Old physicians well remember the time when prescription-writing was taught as quite an art. Now simplicity seems to be especially cultivated, at least this is true of the practice of the best physicians of the day, and the prescriber is content, it may be, with one "adjuvant" or "corrigent," and almost any convenient "vehicle." The long, often heterogeneous and absurd recipes of a former age are no more heard of, the ingredients of the prescription are few in number, and "direct medication" is attempted.

Pharmacists in many places complain that there is not so much call for the old-fashioned tinctures and fluid extracts of the Pharmacopœia as formerly, and that the tablet triturate with its single drug and definite dose is usurping the place of the classic preparations. This is not the place to discuss whether this is an improvement or an evil.

Legibility in prescription-writing is just as much a desideratum as ever. The melancholy accident will not soon be forgotten (it occurred in Paris about two years ago), where a medical man wrote for one gramme of antipyrin; the chirography was so execrable that the word *antipyrin* was mistaken by the druggist for *atropine*, and a gramme of the latter was administered, with resulting death of the patient. There can be no excuse for the slovenly manner in which some physicians write their prescriptions.

Shall the physician write his own formulæ, or shall he depend on the formulæ of his textbooks or the medical journal he takes? We think that, as a rule, he will do better to devise his own formulæ, which should be the result of an intelligent study of each individual case, and a careful adaptation of the drugs to the patient, with the intent to obtain a definite therapeutic effect. He who borrows all his formulæ is likely to prove a careless and inefficient practitioner.

A NEW VOLATILE ANTISEPTIC.

IN the *Internationale Klinische Rundschau* for September 6, 1891, Dr. A. Passerini writes that some months ago he began the treatment of affections of the bronchi and lungs through inhalations of trichlorophenol, which, as is well known, is an active antiseptic, but, on account of its extremely unpleasant odor and highly-irritating qualities he was compelled to suspend his experiments. Recently, however, he states that Dr. Tacchini has succeeded in preparing a pure preparation, which he terms chlorophenol, and which is quite as antiseptic as the trichlorophenol, and yet free from the disadvantages of the latter. This new remedy is stated to have no unpleasant odor, is not irritating, is a highly volatile liquid, and its vapor is heavier than air. Dr. Passerini first satisfied himself of its activity as a local application to wounds, sluggish ulcers, and suppurating lymphatic glands, and then employed it in several cases of ozæna, laryngitis, chronic bronchial catarrh, and especially in cases of pulmonary tuberculosis. The author maintained that the great volatility of this new antiseptic and the fact that its vapor is heavier than air leads to the rapid distribution of the fumes in the bronchi, and that they extend down through the finest bronchi and even to the alveoli, where they are capable of exerting a direct beneficial action.

Dr. Passerini promises shortly to publish a full account of this remedy, especially as regards its composition, physical and chemical attributes, and modes of application, but he now calls attention to the remedy with the idea that the experiments may be carried on by others. He now states that inhalations of chlorophenol are well supported, even by patients in the last stages of tuberculosis; that their employment is entirely free from any disadvantage, even when persisted in for a long time, and that they invariably, as far as his experience extended, lead to favorable results, which are evidenced by the reduced amount of expectoration, sometimes to its entire suppression, to the disappearance of bacilli, improvement or disappearance of cough, disappearance of fever, return of appetite, increase in the body-weight, and, in fact, general improvement.

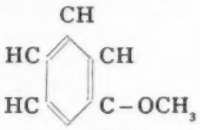
The author claims that he has now treated five cases of incipient pulmonary tuberculosis, and as far as six months' observation will permit, he believes that they are entirely cured.

Reports on Therapeutic Progress.

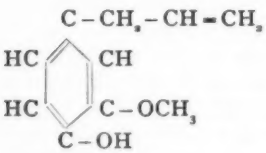
BENZOYL AND CINNAMYL EUGENOL.

It is reported that, in the search for remedies for tuberculosis, experiments are now being made with the benzoyl and cinnamyl esters of eugenol (*Pharm. Centralb.*, June 25, p. 365). It will be remembered that oil of cloves has

already been recommended in the treatment of tuberculous affections, and Dr. Koch has suggested the internal administration of eugenol, its chief constituent, to patients undergoing the tuberculin treatment. Probably the use of the eugenol esters has been suggested by the introduction of the corresponding esters of guaiacol under the names "benzosol" and "styracol." The relation will be seen if eugenol be represented as guaiacol (the methyl ether of pyrocatechin), in which an atom of hydrogen in the para position to the hydroxyl group is replaced by an allyl group.

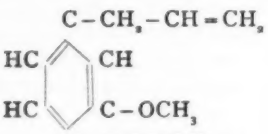


Guaiacol.

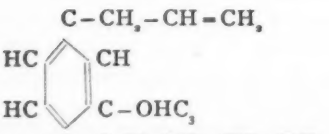


Eugenol.

The benzoyl and cinnamyl ethers of eugenol might then be represented by the formulæ—



Benzeugenol.



Cinnamyl-eugenol.

Benzeugenol is described as crystallizing in colorless and odorless needles, having a weak bitter taste, being neutral in reaction and melting at 70.5° C. It is almost insoluble in water, and freely soluble in hot alcohol, chloroform, ether, and acetone. With concentrated sulphuric acid it gives the purple-red color characteristic of eugenol, this reaction distinguishing it from benzosol, which gives only a pale-yellow color. Cinnamyl-eugenol forms shining needles, which are colorless, odorless, and tasteless, neutral in reaction, and melt at 90° to 91°. It is scarcely soluble in water, but freely in hot alcohol, chloroform, ether, and acetone. With concentrated sulphuric acid it gives a purple-red color, like benzeugenol, but it may be distinguished by the different melting-point. Both compounds are readily saponified with alcoholic potash. —*Pharmaceutical Journal and Transactions*, August 1, 1891.

NEW TREATMENT OF DIPHTHERIA.

The highly unsatisfactory state of the therapeutics of this terrible destroyer of infantile life is assuredly in nowise better shown than by the amount of literature constantly devoted to the subject, and the number of systems of treatment continually being proposed. Pretty nearly every drug in and out of the Pharmacopœia has had its advocates, and still the sheaves are garnered, and the edge of the sickle has not been turned by drugs and systems.

Professor Seibert proposes (*Archives of Pediatrics*, June, 1891) yet another system of treatment of pharyngeal diphtheria, which is interesting from some points of view. Basing his ideas upon the fact that the pharyngeal manifestations of diphtheria begin as a local process, and that this owes origin to the entry and penetration in the mucous membrane of the Klebs-Loeffler bacillus; that the pseudo-membrane is not the disease, but the result of the disease, and is "a safe guide to the diphtheritic inflammation below it;" that the chief treatment should be local, and that the removal of pseudo-membranes is useless, as the bacilli contained therein are of no further consequence, and that local treatment, as carried out generally, does not reach the *active* bacilli in the lower strata of inflamed tissue, and is therefore neither local nor germicidal; that wiping away the pseudo-membranes and applying strong antiseptics to the parts is also ineffective, as only tending to cauterize and infect the healthy surrounding mucosa, to rubbing the bacilli into deeper parts, and is without germicidal effect, Professor Seibert has devised instruments for the purpose of bringing comparatively small, but very strong, solutions into *direct* contact with the bacteria which are in activity upon the lower stratum of the mucosa. The antibacillary medium to be used is the officinal and freshly-prepared chlorine-water of the United States Pharmacopœia, and with a special syringe (the chief feature of which appears to be that instead of one needle-point there are five such points arranged on

a flat disk) the points are pressed firmly in to their full length into the pseudo-membrane, so as to reach the inflamed tissue below, and chlorine-water is injected into the part. Thus brought into direct contact with the active bacilli and cocci of diphtheria, these latter are immediately destroyed, and "the process comes to a stand-still." The contact of the chlorine and the active germs is the foundation of the treatment.

After the injection a gargle of one or two grammes of tincture of iodine, and ten drops of concentrated carbolic acid, in four ounces of water, is given, a teaspoonful being alternately gargled and swallowed every fifteen minutes, from 6 A.M. to 12 at night; five drops for gargling, and half a teaspoonful every half-hour for swallowing, being given to younger children. Zinc and mercurial ointment is rubbed into the swollen glands every two or three hours or less, and an ice-bag adjusted over the swollen parts of the neck. It is claimed that where the process is localized, and the membranes are undermined by the chlorine injections, the temperature makes three to four degrees, and the œdematous swelling disappears. Though the pseudo-membranes remain in the throat for two to four days, they are harmless, but the mouth-wash keeps them from spreading the process. Of thirty-five cases, Dr. Seibert claims to have only lost two under this method of treatment, and none of his patients developed diphtheritic paralysis. If we could be sure that the arguments in favor of the treatment were not of the *post hoc, propter hoc* kind, we might be tempted to echo the author's remarks, that "these cases are sufficient to show that the chlorine-water injections are 'efficient, local, and germicidal' enough to check the career of any diphtheria germs they come in contact with." At all events, the results are good, the treatment novel, and, in view of the disappointing nature of most plans of treatment of diphtheria, we cannot afford to disregard any suggestion, based upon respectable data, for contending with this formidable disorder.—*Journal of Laryngology and Rhinology*, August, 1891.

THE HYPNOTIC ACTION OF THE ANTIPYRETICS.

In the *Deutsche Medizinal Zeitung* for June 5, 1891, PROFESSOR BERNHEIM calls attention to the great similarity in action of the so-called antipyretics,—quinine, salicylic acid, antipyrin, antifebrin, phenacetin, etc.,—not only in their antithermic, but also in their hypnotic action. When the favorable action of salicylic acid in rheumatism was first observed it was designated as a specific for this affection, but experience has taught that the other antithermic remedies produce a favorable effect likewise in the same manner,—that is, they diminish articular pain without influencing the course of morbid processes itself. According to Bernheim, the connection between antithermic and analgesic action is the following: Fever originates through the absorption into the blood of pyrogenic substances, which act as stimulants to the heat-centres, and reduction of temperature can be produced by the elimination of these pyrogenic substances or by direct action on the heat-centre. If the antithermics were capable of actually destroying the ferments in the blood they would then act as abortive agents. This is, however, not the case, according to Bernheim, who will not even admit a specific action for quinine in the case of malaria, since he maintains that the action of this substance on the disease is merely due to its antithermic properties, which are possessed in almost equal degree by the other antithermics. He concludes that the different antipyretics, quinine and salicylic acid included, only produce their action either through antithermic influence on the cerebral centre, and that through antipyretic action within the organism has never been demonstrated. He likewise maintains that the analgesic action is in close connection with the antithermic action, and likewise is produced by influence of nerve-centres, which are probably included in the gray substance of the cortex. In fact, Bernheim states that by the æsthesiometer he has succeeded in producing marked reduction in the sensibility of the skin after the exhibition of quinine. According to this, therefore, the antipyretics are only indicated in fevers in the height of temperature or when they are to be employed for the reduction of abnormally increased sensibility.

THE USE OF MORPHINE FOR THE RELIEF OF CERTAIN URÆMIC SYMPTOMS.

In the *Medical News* for July 11, 1891, DR. H. W. WASHBURN maintains that there are certain uræmic symptoms — convulsions, renal asthma, and severe cephalalgia—for the relief of which morphine is specially indicated. He believes that it relieves distressing symptoms, without adding to the dangers under which the

patient already labors, and thus gives the physician an opportunity to resort to other well-known methods of treatment,—namely: 1, to reduce the supply of poisonous substances in the blood by regulating the diet, by bowel disinfection, and by attention to the functions of the liver; 2, the removal of uræmic poisons by venesection, purgation, or diaphoresis; 3, their oxidation by the use of oxygen or active exercise; 4, the relief of particular symptoms or antagonizing the poisons.

Headache, drowsiness, coma, dyspnœa, vomiting, diarrhœa, disturbances of sight and hearing, muscular twitchings and convulsions are the symptoms which appear, in varying combinations, in that condition to which the term uræmia is applied. These symptoms are the result of the retention in the blood of certain excrementitious substances which the kidneys, in the course of Bright's disease, acute or chronic, have been unable to eliminate. They are an expression of the deleterious effects of these poisonous substances upon the brain and nervous system. That these symptoms are not the result of the retention in the blood of urea has long been known, although under that supposition the term uræmia was applied to them.

The symptoms being thus various and diverse, it seems reasonable to conclude, and experiment has proved, that they are not the effect of any one or two toxic substances, although it is not possible to determine positively what particular symptom is caused by a given excrementitious substance.

In a given case the symptoms observed will, therefore, vary as the proportions of these different toxic substances in the blood vary. In one case vomiting and diarrhœa may be the prominent symptoms; in another headache, asthmatic attacks, or convulsions; in still another, delirium, tinnitus aurium, hemiplegia, coma.

In uræmic poisoning, when convulsions, cephalalgia, or renal asthma constitute the predominant symptoms, we have, with the first two, spasm of the arterioles of the cerebral cortex; in the last, spasm of the pulmonary arterioles, and, at the same time, rapid and ordinarily feeble pulse, low blood-pressure, dilated pupils, and diminished secretion of urine. In the case of renal asthma there is, as stated, spasm of the pulmonary arterioles; as a result of the consequent dyspnœa there is imperfect aeration of the blood; the abnormal blood, acting upon the vaso-motor centres, and also upon the blood-vessels themselves, causes general arterial constriction and constriction of the renal vessels (as is shown by the actual reduction in the size of the kidney), and hence diminution in the urinary flow. The quantity of urine secreted in a given time is directly dependent upon the blood-pressure; and hence the amount of urine will be increased by any condition that increases the general blood-pressure, unless there is at the same time constriction of the renal arteries. The objects, then, to be accomplished under such condisions as these are: To relax the spasm of the arterioles of the lung and of the cerebral cortex; to diminish nervous sensibility, both sensory and motor; to increase the general blood-pressure while relieving the constriction of the vessels in the kidneys, thus increasing the urinary secretion. Have we a drug in the materia medica the exhibition of which can reasonably be expected to fulfil these various indications?

In considering the physiological effects of morphine, in full therapeutic doses, it will only be necessary to inquire into its effects upon the nervous and circulatory systems and upon the action of the kidneys. Its effects upon the nervous system are to lessen sensibility, both sensory and motor, thus relieving pain and relaxing spasm. Acting upon the circulatory system, it at first produces temporary acceleration of the pulse-rate, soon followed by slowing and increased fulness and force as a result of the action of the drug upon the inhibitory cardiac nerves. The blood-pressure is thus increased. There is no unanimity of opinion as to its action upon the kidneys, some authors affirming that it increases the flow of urine, and others that it diminishes the flow. At all events, it is perfectly clear that it sometimes does increase the amount of urine, and that it must do so by its action upon the blood-pressure and by the relief of local arterial constriction. When retention of urine follows the use of morphine, it results from the blunted sensibility of the bladder. Morphine increases the excretion of urea in man and contracts the pupil.

We thus have *à priori* the clearest and strongest possible grounds for administering morphine for the relief of the conditions named,—convulsions, real asthma, cephalalgia. The indications sought to be fulfilled—relief of spasm, alleviation of pain, increase of blood-pressure, and hence increase in amount of urine—are exactly met by the known physiological effects of the drug. At this time, however, we are fortunately not limited to *à priori* reasons for anticipating favorable results from the use of this drug; there are abundant facts *à posteriori*

to substantiate the validity of the deduction that, the conditions indicated being present, neither in acute nor in chronic uræmic intoxication will morphine add to the dangers already existing, but that it will uniformly produce happy results.

TREATMENT OF BRONCHO-PNEUMONIA IN CHILDREN BY SUBCUTANEOUS INJECTIONS OF THE HYDROCHLORATE OF QUININE.

In the *Journal de Médecine de Paris*, June 21, 1891, DR. ST.-PHILIPPE calls attention to the difficulty in diagnosis between pneumonia complicated with enteritis and typhoid fever, while from the point of view of treatment he points out that there are two features which are valuable indications as to the methods to be pursued. First, as to the degree of bronchitis; and, second, as to the extent of the pulmonary lesion, for the congestive process may be so sudden and extensive as to produce death in a few hours. Against this latter form of danger Dr. St.-Philippe maintains that there is no drug which can equal quinine, whether administered by the mouth, the rectum, or under the skin. Sulphate of quinine he gives in black coffee, while in very small children or those who refuse to take medicine it may be employed in enemata or in powders; but both these modes of medication are slow in action, and for entire reliability he usually recommends the employment of quinine in subcutaneous injections, making use of a solution of chlorhydrate of quinine and equal parts of glycerin and water. Quinine thus forms his main reliance in the treatment of bronchopneumonia, to which may be added blisters, which he even allows to pass on to suppuration over the localities where râles are most abundant. In suffocative catarrh he has recourse to mustard plasters, large blisters or leeches, according to the age of the child; while in some cases he employs tincture of aconite root in doses of 5 to 20 drops in twenty-four hours, and in syrup of aconite in cases where the bronchitis is intense, while quinine and punch are given when the general state is at all depressed. In states of excitement he does not advise the use of opium, but warm baths and small doses of antipyrin. In severe cases, quinine and aconite are to be abandoned and caffeine given subcutaneously, and reliance placed upon digitalis and alcohol as stimulants. He likewise states that oxygen inhalations have given better results than injections of ether.

A NEW TREATMENT FOR CORYZA.

Through the action of carbonic acid under high temperature and pressure on alpha-sodium naphthol an acid bearing the name of alpha-oxynaphthœic acid occurs in the form of white odorless crystals, with a sharp taste, and which are claimed to possess five times the antiseptic energy of salicylic acid. According to the *Pharmaceutische Post* for July 19, 1891, this remedy has been employed with great success in the treatment of nasal catarrh. Fifteen grains of the powder are warmed, and the fumes are then to be inhaled. At first the catarrh becomes markedly increased from the irritating effects of the remedy, but after a few hours undergoes rapid improvement. Ordinarily it is claimed that one employment of this remedy is sufficient.

FATTY DEGENERATION OF THE HEART IN DIPHTHERIA.

During a recent epidemic of diphtheria in Göttingen many persons died, and SCHLEM gives, in the *Archiv für Pathologische Anatomie und Physiologie und für Klinische Medicin*, an account of his examination of the muscular tissue of the heart in the fatal cases. His observations were not confined to those cases that were accompanied with cardiac symptoms, but embraced all the fatal cases that occurred during the time that his research was in progress; thirteen cases in all were the basis of this study. This systematic method of inquiry afforded the writer of the paper an unusual opportunity of estimating the frequency with which the heart is attacked in diphtheria, as well as the nature and extent of the attack. The pathological changes noted were the following: Fatty and granular degeneration of the muscular fibres, slight hyaline degeneration and atrophy, and, in a few cases, extravasations of blood in the form of dots. The connective tissue frequently appeared somewhat rich in cells. Fatty degeneration and granular degeneration were found in every case, although they were very slight in some of them, especially when the false membrane had been the cause of death; in others the degeneration was extreme, so that some fibres seemed to be entirely replaced by fat-cells, affording an explanation of the cardiac enfeeblement noted in those cases, or most of them, during life. Age appeared to have no special relation to this cardiac degeneration, but the duration of the attack seemed to have exerted a marked influence. In cases that exhibited the most extreme morbid changes the disease had lasted from ten

to eighteen days; in other instances, where the fatal event had come earlier, the changes were less extensive, while in cases where the larynx was involved and the patient had succumbed in the early stage of the disease, the least amount of morbid alteration was discovered. The changes in the muscular tissue of the heart seemed to have no influence over pulmonary complications. In several cases in which broncho-pneumonia was present there was little disease of the heart, while in others there was no pneumonia, but extensive cardiac degeneration. In one case proliferation of nuclei was observed in the intermuscular connective tissue, and this lay in close proximity to the pericardium at a point where that structure was undergoing a like change. In several instances the writer found the intermuscular and circumvascular connective tissue infiltrated with nuclei to some extent, but he was not able to determine whether or not this was a normal phenomenon in children. As to the causative influence of fever over fatty substitution, he expresses the conviction that it is not the cause, since in many cases the bodily heat had been moderate or not above the normal point. The pathology of the changes in the heart during diphtheria is analogous to the condition described by Orth as parenchymatous or degenerative myocarditis, which is a frequent result in other severe diseases of an infectious nature.—*New York Medical Journal*, July 4, 1891.

SUBCUTANEOUS INJECTIONS OF IRON.

DR. ROSENTHAL has made an interesting study of the action of subcutaneous injections of iron in nervous affections. By personal experiment he ascertained that when a preparation of iron was administered subcutaneously, it was absorbed thirty to forty minutes after injection, and could be detected in the urine. From the basis of a long series of investigations the author recommends two iron preparations for hypodermic injection. One is the so-called ferrum peptonatum, obtained by the decomposition of ferric chloride solution with solution of pepsin, as a brownish-yellow powder, readily soluble in water. It is applied in aqueous solution (1 to 10), one syringeful being given every second day. The second suitable preparation is ferrum oleatum diluted to 1 to 20 by olive oil, and used similarly. Of the two, the former is preferable, on account of its greater solubility and stability. The subcutaneous iron treatment is recommended by Rosenthal in delicate neurasthenic persons and in the asthenic dyspepsia so often associated with anæmia, in which small doses of iron may produce considerable digestive disturbance. Unpleasant by-effects did not appear.—*Provincial Medical Journal*, September 1, 1891.

PHENOCOLL HYDROCHLORATE.

In *Notes on New Remedies* for August, 1891, DR. ISAAC OTT publishes the following summary of his experiments made with hydrochlorate of phenocoll.

Upon frogs it produces a general paralysis, due to an action upon the cerebro-spinal axis.

Upon rabbits it produces a cyanotic condition of the ears, and reduces the force and frequency of the heart.

It kills through an action upon the centre of respiration.

Upon the vascular tension the tendency is to diminish it.

Upon the temperature its action is one of reduction.

Its effect upon the temperature seems to be of short duration.

These experimental facts explain its utility in rheumatic pains and pyrexia.

It is evidently a drug of considerable power and rapid in its activity. This drug seems to be worthy of a fair trial at the hands of the profession.

GONORRHŒAL SALPINGITIS.

TERRILON (*Gazette de Gynecologie*), in a valuable contribution upon this subject, in speaking of the treatment, says that the curette can only modify the uterine mucous membrane, which is at the same time affected, and in this way may relieve the patient somewhat, but the tube remains in the same state. The medical treatment is symptomatic. He regards rest in bed as an important measure; next to this he values vaginal irrigation. The patient is placed in position as for examination with the speculum, and a certain quantity of warm water is injected, and, as soon as the patient complains of too much heat, the flow is arrested by obstructing the vulvar orifice. After several minutes the water is allowed to flow out, and the process is repeated. This is continued for a long time. He has had good effects from blistering the abdomen with blisters or the hot iron. Constipation is to be relieved by repeated purgation, and pelvic peritonitis is treated by absolute repose, cataplasms, revul-

sives, and leeches. Such measures, he says, can ameliorate very much the condition of the patient and even bring about the semblance of a cure, but we cannot be sure that the patient will not have a relapse, even after a very long period of absolute freedom.—*Archives of Gynæcology*, August, 1891.

INFLUENCE OF ANTIPYRIN ON THE SECRETION OF MILK.

DR. GUIBERT writes in the *Lyon Médical* for August 9, 1891, that antipyrin renders marked service in cases where it is necessary to arrest the secretion of milk from newly-delivered females, provided the kidneys be normal. He states that generally it suffices to administer thirty grains of antipyrin daily for two days, in order to obtain the desired effect. He gives the remedy in capsules of 4 grains each, one to be taken every two hours.

A NEW TREATMENT OF GRANULAR CONJUNCTIVITIS.

The usual treatment of granular conjunctivitis consists in turning back the eyelids with the fingers, and then cauterizing the portion of the conjunctiva thus exposed with the sulphate of copper. This daily cauterization is extremely painful, and does not produce cure until after many weeks of severe suffering, and sometimes did not prevent absolute blindness.

At the meeting of the Académie de Médecine, held on August 4, 1891, DR. ABADIE described a method in the first place of completely everting the upper eyelid so as to expose the superior conjunctival cul-de-sac (*La France Médicale*, August 7, 1891). In the ordinary method of procedure it is this part of the superior cul-de-sac which is never exposed, and which is ordinarily a veritable focus of infection. To accomplish this complete inversion of the eyelids special instruments are necessary, and the patient must be chloroformed on account of the severe pain produced by this manœuvre. The eyelid is seized with forceps, and then rolled twice upon itself, or, in other words, rolled twice completely around the forceps, so as to completely expose the superior conjunctival cul-de-sac, which is then scarified freely with a bistoury, and a fairly hard tooth-brush is then dipped in a solution of corrosive sublimate, 1 to 500, and the raw surface vigorously brushed with this solution. This procedure is only required once, and then on subsequent days the eyelids are simply everted in the ordinary manner and the conjunctiva bathed with a solution of sublimate. The author states that this method of treatment may be applied to all cases, no matter what may be the existing degree of complication, and that its efficiency is the more marked the more serious is the case.

Dr. Guaita recommended first the use of sublimate in strong doses, Sattler insisted upon the efficiency of scarifications combined with scraping of the granular tissues, while Manolescu suggested the use of the brush; but the value of the procedure depends upon the employment together of all these procedures, which individually are inefficacious, and upon the complete exposure of the superior cul-de-sac.

THE INFLUENCE OF ENEMATA AND CERTAIN REMEDIES ON THE COMPOSITION AND QUANTITY OF THE BILE.

DR. A. LEVENTON (*Les Nouveaux Remèdes*, August 8, 1891) has experimented on a dog with a permanent biliary fistula. He has determined that enemata of warm or cold water have no action on the quantity or composition of the bile.

Gamboge in small doses causes inactivity. In large doses it diminishes the quantity of the bile. In the absence of bile from the intestinal tract the purgative action of gamboge is reduced.

Jalap, or its active constituent, jalapin or convolvulin, are without influence on the bile. Jalap and convolvulin lose their purgative power when the bile is absent from the intestine. This is not the case with aloes and aloin or jalapin.

Aloes and aloin are not to be considered as cholagogues. This also applies, according to the author, to podophyllotoxin.

A NEW MODE OF ADMINISTERING THE BROMIDES.

The *Pharmaceutical Record* remarks that in Paris the pharmacists have of late been astonished by the increasing number of prescriptions they have been called upon to dispense wherein the bromides are combined with naphthol and bismuth. This new departure is simply the practical carrying out of some suggestions made last year by Professor Féré, of the Salpêtrière, that large doses of the bromides tended, in certain individuals, to beget unpleasant symptoms chiefly for the reason that the gastro-intestinal tract

of such persons was in a condition of sepsis that prevented the proper assimilation of the drug. He recommended the administration of such intestinal antiseptics as naphthol and salicylate of bismuth as a means of removing drug intolerance from this and from other causes. The following formula is one method found by him to be advantageous, in the treatment of epileptics especially:

℞ Potassium bromide, ʒiss;
Beta-naphthol, ʒi;
Salicylate of sodium, ʒss. M.

Sig.—Divide into three doses, one dose to be given three times daily.

It is maintained by Féré that this treatment is curative as well as preventive. He has found that the eczema and psoriasis which sometimes follow in the train of borax will also disappear if the intestinal tract is rendered aseptic. To the formula above given some Paris physicians are in the habit of adding one-thirtieth of a grain of sulphate of strychnine.—*New York Medical Journal*, September 5, 1891.

THE ACTIVE PRINCIPLE OF WOOD ANEMONE.

Dr. Dupuy, in *Les Nouveaux Remèdes*, July 8, 1891, has extracted, in the form of crystalline needles, a substance from the wood anemone which seems to possess extremely energetic action on the economy. In large doses its toxicity is very marked, producing trembling, hebetude, and sanguinolent diarrhœa, and death from paralysis. From the therapeutic point of view, this drug, which he calls "anemonine," is said to act with great efficiency in catarrh and chronic bronchitis, and especially as a calmative of convulsive cough and in whooping-cough. It is also said to possess emmenagogic properties.

NEW REMEDIES IN WHOOPING-COUGH.

Von Genser was the first to prescribe the use of antipyrin in the treatment of whooping-cough. His cases amounted to one hundred and twenty in number, and he stated that under its administration he found that the duration of the disease was reduced as a rule to twenty-four days, instead of its usual length of sixty to ninety days. He employed it in doses of 1½ grains to children of 3 or 4 years of age.

Dr. Sonnenberger has used it in seventy cases, giving to very young children ½ to 1½ grains four times daily. He likewise claims considerable improvement in the severity of the attacks and marked reduction in the duration of the disease. Nevertheless the administration of antipyrin is not absolutely harmless, and cases of poisoning from this substance are not rare. Thus, Dr. Rothe claims that he has seen numerous unfavorable results following the use of antipyrin, and that he now makes use of a combination of tincture of iodine and carbolic acid, which he recommends in the strongest terms in the treatment of whooping-cough. He writes that in one hundred cases occurring in his own practice, in which there was no possibility of error in diagnosis, the maximum duration of the disease was four weeks. He makes use of a solution containing carbolic acid and alcohol, each 1½ grains; tincture of iodine, 10 drops; tincture of belladonna, 30 drops; mint-water, 2 ounces; of which a teaspoonful may be given every hour to a child 2 years of age. Antipyrin has also been employed with advantage as a sedative and antispasmodic in whooping-cough. Dr. Love has published fifty cases treated in this manner with satisfactory results. Dr. How has likewise employed acetanilide with success, the dose given four times daily, varying with the age of the child, from ½ to 5 grains. Dr. Heiman also recommends the use of phenacetin in whooping-cough, and he mentions a case treated with good results with phenacetin when antipyrin had been without effect. He administers 1½ grains four times daily to a child 3 years of age. We have also before alluded to the employment by Moncorvo of resorcin in whooping-cough, and his method has been followed out by Bouchut, Callais, and Mauriac, who claim that they have been satisfied with the results obtained. On the other hand, Mugdan writes that in eight cases he employed resorcin, and in all without effect.

In conclusion, we may call attention to the fact that sulphurous acid has been employed by Mauby, bromoform by Stepp, Löwenthal, and Neumann, chloroform-water by Stepp and Shilling, hydrate of terpin by Manasse, while finally ouabaine has been recommended by English physicians.—*La Pratique Médicale*, June 23, 1891.

PANCREATIC DIGESTION OF FATS.

Last week we referred to the work of Gad and Rachford on the formation of spontaneous emulsions. They found that if a drop of oil containing five and a half per cent. of fatty acid was dropped upon a one-fourth per

cent. sodium carbonate solution, a spontaneous emulsion was formed. Using this method in the investigation of the pancreatic digestion of fats, Rachford discovered some very interesting and valuable facts. Discouraged by Heidenhain's attempts to make permanent pancreatic fistulæ in dogs, he made use of temporary fistulæ in rabbits, in this way always securing fresh juice for his experiments. The portion of the duodenum containing the end of the pancreatic duct was resected, brought to the surface of the abdomen, and the pure juice obtained through a glass canula inserted into the open end of the duct. Taking the pancreatic juice so obtained, it was shaken up with neutral olive oil for a moment, and then the two allowed to separate. A drop from the supernatant oil was then pipetted off and dropped upon a one-fourth per cent. solution of sodium carbonate. This operation was repeated every three minutes until a perfect spontaneous emulsion was formed. As an average of his experiments, he found that about twenty minutes were required to form the perfect emulsion under these circumstances; or, in other words, the juice would produce five and a half per cent. of fatty acid in the oil in twenty minutes. These experiments were conducted at the ordinary temperature of the room, 18° C. (64.4° F.). When the mixture of the oil and juice was kept at the temperature of the body, the fat-splitting activity of the juice was doubled, and the five and a half per cent. of fatty acid was formed in about ten minutes. The emulsions formed in this way had all the peculiarities of the emulsions formed with neutral oil, to which a definite amount of fatty acid had been added, and sodium carbonate solution, to which we referred last week. The most important feature of his experiments, perhaps, was his determination of the influence which the other digestive agents had upon the fat-splitting properties of the pancreatic juice. Representing the activity of the juice at room temperature by 20, the addition of dilute hydrochloric acid was found to reduce this activity to 13, while the addition of the principal constituent of the bile—viz., the glycocholate of soda—increased it to 44. When the glycocholate of soda and hydrochloric acid were both added, the activity was still further increased, being represented by 56. That the other ingredients of the bile were not unimportant in this connection was shown by the fact that the juice and bile alone had an activity of 64. The greatest effect, however, was obtained by a mixture of pancreatic juice, bile, and hydrochloric acid, the activity of the mixture being 80, or four times that of the pancreatic juice alone.

Rachford's experiments show very clearly that the bile does not assist in the emulsification of fats, as has been so long taught, but, on the contrary, directly interferes with the emulsification, while it does assist powerfully in decomposing the fats into glycerin and fatty acids.

It has been the custom of late to study the digestive ferments separately, in order to obtain so-called accurate information, but these experiments certainly indicate that the combined action of these ferments may give strikingly different results from what their separate actions would indicate.—*Journal of the American Medical Association*, August 22, 1891.

INJECTIONS OF NITROGLYCERIN FOR POISONING BY ILLUMINATING GAS.

In the *Repertoire de Pharmacie* for August 10, 1891, reference is made to a case reported by Dr. Hoffmann of an actress to whom the author was summoned, and in whom he found the face pale, the extremities cold, foam on the mouth, pulse filiform and intermittent, respiration superficial and slow, and the cornea insensitive. It was determined that inhalation of illuminating gas was the cause of this serious condition, and, injections of ether producing no improvement, Dr. Hoffmann injected $\frac{1}{64}$ grain of nitroglycerine into the præcordial region, whereupon prompt improvement took place, and the patient recovered.

THE APPLICATION OF HOT-AIR-TAR BATHS IN THE TREATMENT OF RHEUMATISM.

Dr. Paul Chevandier has written an interesting thesis on this subject, from which we extract the following: Hot-air baths were first used in Oriental countries, and introduced into Europe by the Moors. It appears that the natives of Mexico, at the time of the discovery of America, also employed heated air saturated with aromatic vapors in the treatment of rheumatic pains. The aborigines dug a hole in the ground large enough for a man to lie in, and over this built a sort of dome of stones and clay, the whole structure looking somewhat like a baker's oven. The bottom was covered with aromatic shrubs, on which the sick man lay.

In France ovens in which resinoid matters were burned were employed in early times for the treatment of rheumatism. Later the oven was replaced by a "fumigation chamber." In the latter a fire was built, which heats the air and impregnates it with resinous vapors, which are conducted to several chambers communicating with the furnace.

According to M. Chevandier, there is little or no analogy between the physiological effects of dry thermo-resinous vapors and those of ordinary steam vapor baths. The former produces redness, congestion, and tension of the skin, the latter soften and relax it. The diaphoresis produced by the former results from the increased activity of the sudoriparous glands, while the sweating from the latter is due simply to a passive exudation. In dry-air baths one can readily endure a temperature of 70° to 80° C., while in the ordinary steam bath a temperature of 50° C. is very uncomfortable, and 55° C. causes a burning sensation.

In hot-air-tar baths of a temperature of 60° C. the patients perspire freely and absorb large amounts of oleo-resinous vapors, as is shown by the strong odor of violets emitted by the urine. A short time after the bath the perspiration becomes so abundant that some patients lose as much as a thousand grammes of bodily weight within half an hour. A miliary eruption may be developed, but this will disappear spontaneously within twenty-four hours. Headache is observed in rare instances, but yields readily to the application of cold to the head. The digestive functions are stimulated. Some nervous excitement, insomnia, and irritability are observed for a short time after the bath, and sometimes a transient recurrence of the rheumatic pains. The hot-air baths are serviceable in the different forms of apyretic rheumatism. The profuse diaphoresis is never debilitating, but the duration and number of the baths should not exceed the limits assigned to hydrothermic treatment.—*Journal of Balneology and Dietary*, August, 1891.

THE ACTION OF HYDRASTINE UPON THE VASCULAR SYSTEM AND THE UTERUS.

Authorities are divided (*Les Nouveaux Remèdes*) upon the mode of action of this drug on the vascular system. Some have observed an increased blood-pressure and slowing of the heart's action, while others affirm that the drug paralyzes the vascular system. The uterine contractions provoked by hydrastine are due, according to some, to the direct stimulation of the muscles and of the cardiac nerves; according to others, to the stimulation of the central nervous system. It was for this reason that Serdsteff had undertaken new experimental researches upon frogs (sixty-two experiments) and upon warm-blooded animals (fifty-seven experiments). These are the results obtained by this experimenter:

Given in a small dose, hydrastine provoked, in frogs and in warm-blooded animals, slowing of the heart's movements, owing to a stimulation of the inhibitory apparatus, both peripheral and central. Small doses always increased blood-pressure; by larger doses it was lowered.

These differences depended upon the state of the vaso-motor centre. Small doses were not followed by convulsions, and did not paralyze either the respiration or the heart. He thinks, then, that if some observers have verified the paralyzing action of hydrastine upon the vascular system, it must be those who have given larger doses of the remedy. Regarding the influence exercised by hydrastine upon the uterus, the voluntary or rhythmic contractions of this organ would be increased in their strength, their number, and their duration.

Hydrastine does not act *directly* upon the neuro-muscular apparatus of the uterus, but *indirectly* by the intervention of the central nervous system, and that very probably by way of the vaso-motors.

In sustaining these obtained results, the experimenter counsels the use of hydrastine in all those cases of hemorrhage where it would be, for one cause or another, unwise to await strong contractions of the uterine muscles, and where it is of the greatest importance to attack the bleeding through the vessels of the uterus.—*Archives of Gynæcology*, August, 1891.

EFFECTS OF MEDICINAL AGENTS UPON THE COMPOSITION AND REACTIONS OF THE URINE.

The urine of persons to whom salol has been administered precipitates the red oxide of copper when tested with Fehling's solution. With the polariscope, the urine in such cases polarizes to the left. It is observed that, if the urine should at the same time contain glucose, the reading by the polariscope would be too low, and the result would indicate a smaller proportion of sugar than is really present. In order to distinguish the salol urine from saccharine urine, the follow-

ing process is recommended: "A tube of the capacity of fifteen cubic centimetres is half filled with the urine, previously treated with basic acetate of lead. To this are added five centigrammes of hydrochlorate of phenylhydrazin and twenty centigrammes of sodium acetate. The liquid, which assumes a yellow color, is heated in a water bath to 100° for half an hour, and then allowed to cool, when the precipitate formed is examined microscopically. If due to derivatives of salol, the precipitate is amorphous; if to sugar, it is composed of small silky crystals of phenylglucosazone, which are at all times grouped in tufts." Hischl considers this phenylhydrazin test a perfectly trustworthy one. If the urine gives typical needles of phenylglucosazone, it certainly contains glucose. It is essential, however, that the test-tube should be allowed to remain one hour in the water bath. If this is done, the precipitate caused by glycosonic acid, if this substance should be present, is wholly amorphous and easily distinguishable from phenylglucosazone. Levulose cannot be distinguished by this test, but requires the polarimeter. Lactose found in the urine of nursing women also furnishes needle-shaped crystals which are about ten times the width of phenylglucosazone, and do not show the same orderly arrangement. Their melting-point is 200°. Maltose gives a precipitate which occurs in yellow tablets, melting at 82°.

Hæmatoporphyrin has been noticed in urine, giving it a port-wine color, without being accompanied by albumen, sugar, bile, or blood. Salkowski noticed this occurring in three cases, and attributed the presence of the pigment to the fact that the patients were at the time taking sulphonal. V. Harley and S. Torup report a case of a woman treated by chloral and sulphonal, in which the urine was of this character. R. Stockman states that after the administration of balsam of Peru no disturbance of the kidneys results, but the urine gives a precipitate with nitric acid resembling that due to the presence of albumen, except that it is soluble in alcohol. This also has been noticed after the administration of copaiba.—*Journal of Balneology and Dietary*, August, 1891.

THE TREATMENT OF DIABETIC COMA.

In a recent number of the GAZETTE we called attention to the treatment of diabetic coma through the intravenous injection of saline fluid containing chloride of sodium, chloride of potassium, sulphate of sodium, phosphate of sodium, and bicarbonate of sodium, as suggested by Dr. Hilton Fagge and extended by Dr. Dickinson. In the *Medical Chronicle* for August, 1891, DR. ERNEST S. REYNOLDS writes that in the last twelve years he has seen about twenty cases of diabetic coma, all proving rapidly fatal with the exception of the last two, which were treated by the method of Dr. Dickinson, with the exception that, instead of using transfusion, he used the stomach for the introduction of the saline liquid into the blood. This was quite feasible, as all the successful cases were not absolutely comatose. An aperient was ordered, and the patient was directed to drink during the night at least a gallon of fluid, together with an ounce every hour of a mixture containing sixty grains of potassium to the ounce. The next day the patient was slightly better, and the treatment was continued for twelve hours, the citrate of potassium being lessened in quantity. In three days all symptoms of coma had passed away, the sugar had again increased in quantity, the acetone had disappeared from the breath and the urine, and the albuminuria had entirely ceased.

The second case was somewhat similar, and was treated in much the same way, and likewise was relieved of the threatened symptoms of coma, although, of course, his diabetes likewise continued.

The point which is especially worthy of notice is that as probably no case of recovery from diabetic coma is known, it is absolutely necessary to diagnosticate impending coma in its earliest stage, and to so deal with it as to prevent unconsciousness coming on. The chief points to be noticed in this early stage are: A distinct sense and appearance of increased illness, often with loss of appetite, increased weakness, slight drowsiness, pain in the left hypochondrium, a labored respiration, the expiration being especially prolonged, an acetone-like smell in the breath and urine, lessened excretion of sugar, so-called acetone reaction (port-wine coloration with perchloride of iron) in the urine, and albuminuria. When these symptoms are present the patient is in the greatest danger.

The treatment advocated is as follows: Absolute rest in bed, purgation (but not to an excessive degree), a slight relaxation of the diabetic diet, large doses of citrate of potassium, and very large quantities of fluid taken internally. These fluids may consist of milk, lemonade, tea, water, or even barley-water, a

variety being necessary in order to induce the patient to take a sufficient total quantity, which should amount to nearly a gallon in twelve hours.

Reviews.

LECTURES ON TUMORS. By John B. Hamilton, M.D., LL.D. The Physician's Leisure Library.
Detroit: Geo. S. Davis, 1891.

The above work is a course of clinical lectures delivered by the author to his class of students, and, as an introduction to the study of this most interesting and important subject, in connection with the study of medicine, it will well repay a careful perusal before the reading of more complete and elaborate works are undertaken. The author has, in a brief *résumé*, given the outlines of his subject, and while we are not advocates of such short treatises upon such a large subject, yet as a foundation to build upon we can advise Dr. Hamilton's little work as one which will interest and instruct the reader.

J. H. C. S.

PRACTICAL INTESTINAL SURGERY. By F. B. Robinson, B.S., M.D. The Physician's Leisure Library. Vols. i., ii.
Detroit: Geo. S. Davis, 1891.

Our author makes the statement that "no man should enter the abdominal cavity who has not trained himself for the work." In our opinion the above quotation is equally applicable to all surgical operations, and a special training in any procedure of a surgical nature must necessarily improve the technique of the operator; therefore, while admitting that the frequent repetition of any or all surgical operations tends to perfect the operator, yet we are not inclined to exclude any surgeon from performing any operation, provided he has given the necessary study and practical manipulation to the operation. In a word, we are not believers in exclusive specialism; but judging surgical operations from the stand-point of a thorough surgical knowledge and experience, their performance is not to be exclusively confined to any special surgeon.

The two small volumes of Dr. Robinson are an experimental pathological study of "Intestinal Surgery." The number of experiments reached one hundred and seventy-five, and the conclusions drawn from this large number of difficult operations upon the abdominal cavity have not only very materially added to our knowledge of the subject, but they have also given us many improvements in methods and technique. The work is a decidedly valuable addition to surgical literature.

J. H. C. S.

LE VÉGÉTARISME ET LE RÉGIME VÉGÉTARIEN RATIONNEL, DOGMATISME, HISTOIRE, PRATIQUE. Par le Dr. Bonnejoy (Du Vexin), etc. Précédé d'une Introduction par le Dr. Dujardin-Beaumetz.
Paris: Librairie J. B. Bailliere et Fils, 1891.

As an indication of the undercurrent of thought, philosophical and theological, one may point to antivivisection and vegetarianism as showing the deeper stream that is daily and hourly sweeping into its flood more of the thoughtful and earnest, whether it be better to eat flesh or worse to kill the brute to obtain it is the question, and a certain grade of mind, after reaching a certain point, drifts towards the vegetable diet from sheer necessity and consistency.

We have in this book brought together all that can be said in favor of a vegetable diet, and all and more than all that can be said against the eaters of meat. They will find themselves reviled and berated soundly in its pages. "Corpse-eaters" is a mild form of opprobrium which the author frequently bestows upon them; but he has dragged together every argument that he can find or invent, without regard to the force of it or to its truth. How far astray he goes will be readily seen when we find him actually inverting and reversing the biblical account of Cain and Abel to prove his point. Cain should have been a bloody corpse eater and his sacrifice should have been animal, for he was a murderer; and poor innocent Abel should have meekly offered the fruits of the earth, so he makes him do so. Possibly Dr. Bonnejoy is one of the fatal first fruits (French) of the higher biblical criticism.

The author supplements his vegetables with milk and eggs, and kindly allows us to cook them, but fails, we think, to prove that the energy and restless activity of modern life can be sustained by such a diet. While we are ready to grant that the civilizations of the East—the meditative philosophers and the native hordes who live a totally different life from ours—may succeed in subsisting on it, and if, as the author endeavors to prove, most of our diseases are due to meat diet, surely most of our extra resistance to disease comes from the same source. And whence, we would ask, come the diseases of the vegetarians? India is the home of cholera, and the home

of vegetarianism also; while many European physicians have remarked upon the ease with which the natives of Eastern climes succumb to disease, almost without a struggle. If we admit that the Eastern civilization is superior to the Western,—that we are all wrong in our ways,—and if we can convince and convict the Western world, then, with the total abolition of progress and all that it means, we may go back to a diet of rice and beans. Our great discoverers will become great philosophers, and instead of the busy stir of Wall Street quiet will settle down, and where once commerce jostled and crowded, half-clothed sages, in dirt and neglect, will sit in the sunlight and con the problems of existence. As it is, however, we let them do that sort of thing in Germany. E. W. W.

THE PRACTICE OF HYPNOTIC SUGGESTION, BEING AN ELEMENTARY HAND-BOOK FOR THE USE OF THE MEDICAL PROFESSION. By George C. Kingsbury, M.A., M.D., Dublin.
Bristol: John Wright & Co., 1891.
London: Simpkins, Marshall, Hamilton, Kent & Co. (Limited), 1891.

This is a very timely book, and one that comes to us with authority. Not only the methods of inducing hypnotic sleep, the nature of the cases likely to be benefited, are given, but the book is in good part devoted to a refutation of the reckless arguments that have been used against hypnotism. The author admits certain dangers and denies others in toto. In the case of the admitted dangers he suggests what he considers perfect safeguards against them, or shows that they are no greater if as great as the dangers of chloroform or ether. The part devoted to medical jurisprudence in its relation to hypnotism is excessively interesting; and the author has carried his investigation into minute details, anticipating what will be the difficulties in the future in a very curious way. Some of his details are new and unexpected. He is a strong advocate of hypnotic suggestion, and its powerfully good results in dipsomania and general moral misdirection. He claims that there is no evidence that the mind is ever weakened, but always the better for suggestion.

Of Dr. Luys's transfer experiments he has, after careful investigation and repetition, very little opinion, although he considers Dr. Luys thoroughly honest in his convictions. The "sealed tube" business also seems to him very doubtful, and the magnetized bar experiments fall under the same suspicion. The book is full of illustrative cases. After reading the book through, we incline to the belief that the author's medico-legal speculations are the most valuable part of it, and show better than anything else the immense difficulties of the subject in its relation to the public. In short, back of the hypnotism of to-day there lies a vast unexplored field of knowledge—possibly attainable, possibly not—of sinister import as it looks at present, but possibly of incalculable value to the human race. It is no wonder that enthusiasts grasp at it as the great future hope, the vast reformatory agency of the future, before whose suggestive agency all the fixed bad habits, all the results of long-continued evil methods and surroundings will fade away. If one-tenth of what some have claimed for it be ever realized we shall see wonders. It only occurs to us that it was a pity that Omnipotence did not in his deep sleep (perhaps hypnotic) suggest to Adam that on waking he should refuse all fruit, no matter how tempting, or to Eve that she should avoid yielding to any serpentic suggestion whatever.
E. W. W.

THE DISEASES OF PERSONALITY. By Th. Ribot. Authorized translation.
Chicago: The Open Court Publishing Company, 1891.

It will hardly do to discuss hastily such a book as this because it is small and thin. It raises great questions, and to the thoughtful it must prove either a positive help or a positive hinderance. The materialist will read it with joy and place it among his sacred books,—books that help him to prove his theories,—that he came from nothing and to nothing returns, that all his boasted intellect and culture are but the foam on the wave-crest. The spiritualist, in the deeper sense, will see in it the attack of a skilful enemy whose thrusts find out many a weak spot in his armor, and whose attack he will hardly know how to evade. For from its premises, if they are right, the argument is flawless and unanswerable, and neither the orthodox believer, nor the agnostic reader will be able to combat them successfully.

Among the first questions to ask would properly be, Is the author exactly right in his definition of personality, and are personality and the "Ego" identical terms for one and the same thing, or is the Ego more complex? Again, from one possible view, the personality may be as unsubstantial and as fluctuating as the author believes, and still the author may be wrong. In other words, if he has

reasoned fairly, from fairly-observed and recorded facts, his conclusion has a real value, though he himself may not see its exact value in all its relations.

"If the life of every organ comprises two periods,—one of relative rest or assimilation, the other of activity or disassimilation,"—then to prove that the brain is but an organ and consciousness its function, one must prove that in deep sleep consciousness is abolished, to reappear only when its basis is reaccumulated. This the author establishes to his own satisfaction.

The author defines the "Ego." He does not define it from above, like the spiritualist, but goes down to the elementary phenomenon of life for its origin,—organic sense. Hence there is a physical basis for personality formed from the combined organic sensations of the body, united in the general feeling of existence. "It is clear that the exclusive faith which psychology has so long accorded to the nerve data of consciousness would throw into the shade the organic elements of personality;" "but personality," the author argues, "is not a mysterious attribute dropped down from the skies and without antecedents in nature, but organic consciousness stands in inverse ratio to the higher psychic development;" hence personality must vary as its organic bases vary, must be diseased or altered as these are diseased or altered.

"The Ego exists only on the condition of continually changing." Exuberant vitality or depression are the commonest alternating conditions, scarcely to be called morbid. Now, if either condition persists, we have formed "a new physical and mental habitude."

Finally, such a change may occur that the individual seems to himself something like, but not the same as, himself. One will even allude to himself as "it." "Here the previous healthy individual and the present unhealthy one constitutes two distinct phases of personality, or a double personality. The feeling of identity between them is lacking." "The recollection remains, but there is no amalgamation." Thus the author leads up step by step, by cited cases, to double physical and then to double mental personality. "Double personality," he says, "is but an abstraction. Each case asks for a particular interpretation." "Personality being itself complex, its perturbations must be multiform."

The Ego is affirmed to be impenetrable and a perfect unity, but "one Ego cannot be another Ego," because one organism cannot be another organism at the same time. Here the author studies to some purpose double monsters, and still further follows the subject up by studying twins, and at the close of the chapter deduces, "as the organism, so the personality." The reader will find in this book, as he can judge by this time, condensed and clearly expressed, a strong and thorough argument for the materialistic view of personality. "The unity of the Ego is coördination," is the author's conclusion. Personality and the Ego are pretty much one and the same at any one time. Anything further included in the "Ego" he does not believe in because he knows nothing about it. Possibly the very mutations of personality might suggest that while it was truly dependent upon organic bases there might yet be a something, evading physical and psychic analysis, of which the varying phases of personality were but passing attributes, and with this thought we leave the author to the curious reader.

E. W. W.

THE GENUINE WORKS OF HIPPOCRATES. Translated from the Greek, with a Preliminary Discourse and Annotations. By Francis Adams, LL.D., Surgeon. Two volumes in one.

New York: William Wood & Co., 1891.

This is a reprint of a publication of the Sydenham Society, and has also appeared in separate volumes in the "Wood's Medical Library" series. The large volume before us certainly seems more formidable and impressive, but a comparison shows that, except paper and binding, it is the same work, already noticed in this journal. All who would be scholars as well as physicians, who feel the need of all possible culture and knowledge, should, if weak in Greek, read a good translation of our author. It is no longer the thing for the medical man to quote largely from the old masters; quite the reverse. He quotes only from the newest lights, which are farthest off, and one can hardly promise that a reading of the book will really add to his stock of practical knowledge. Hippocrates seems to the modern student to have dreadfully needed a four years' course and some post-graduate instruction, but we are apt to forget how much he observed has passed into the axiomatic knowledge of the profession and belongs to every one, claimed by no one; in short, we would say that the Hippocratic oath is the most valuable part of the book, and can be read and remembered by every medical practitioner with profit.

E. W. W.

SCIENTIFIC MEDICINE IN ITS RELATION TO HOMŒOPATHY. By Professor Theodor Bukady, M.D., of the Buda-Pesth University. Translated from the German by Rudolph F. Bauer, M.D.
Philadelphia: Boericke & Tafel, 1891.

A careful reading of this book, which can be done in a short time, fails to convince us that its title is anything except misleading. It professes to be an examination "of Koch's method" in its relation to homœopathy, and, of course, claims its brilliant (!) results as due to its harmony with the theories of Hahnemann. We are quite willing to believe that a harmony between the Hahnemannian theories and anything mentionable may by ingenuity be proved, but the author goes much further, ventilates his own modifications of the immortal theories in question, and fortifies his position at every point with long quotations from his own writings, which he seems to have studied very carefully. The essay is addressed to laymen, and must have had the desired effect if any laymen were present to hear it, for laymen are a "simple folk," and easily captured if one goes to work the right way. Any one desiring an insight into a certain school of homœopathic practice may get some hints from reading this book. E. W. W.

Correspondence.

LONDON.

(From our Special Correspondent.)

Of all the events of the month, there is none on which a greater interest has been concentrated than on the meeting of the International Congress of Hygiene in London. Never before has there been a more brilliant assembly of scientists, and probably the subject-matter of the discussions has never before been of such vast importance. I think, however, I am not wrong in saying that the crowning triumph of the Congress was the discussion on "Immunity" in the Section of Bacteriology. Here the great men of all nations had their say on this most interesting subject, which was discussed from every point of view. But the key-note of the discussion was undoubtedly given by Dr. Roux in his opening address, and it early became evident that the work would resolve itself into a struggle between those whom we must now call the "humoral" and the "cellular" pathologists.

In introducing the subject of "Immunity, Natural and Acquired," Dr. Roux reminded the section that the great bulk of the work now being done took as its starting-point two subjects with which the name of M. Pasteur would always be honorably associated,—the attenuation of a virus and preventive inoculation. Pasteur had succeeded in replacing the old dangerous method of conferring immunity by inoculation by the safer one of preventive inoculation with an attenuated virus. The methods he had adopted to obtain this attenuation were two,—first, the prolonged exposure of cultures to air at a suitable temperature; and, secondly, the passage of the microbes through the bodies of different species of animals. There were other methods of arriving at the same end, such as the action of heat, antiseptics, compressed oxygen, and light, but the first two methods had been found the most satisfactory. In the early experiments on immunity it was always this attenuated but living virus which had been introduced into the animals; recently it had been found that a similar end could sometimes be obtained by the introduction of the chemical substances produced by the virulent bacteria, without actual introduction of the living bacteria.

Dr. Roux then placed before the section an outline of the doctrine of phagocytosis, associated with the well-known name of Professor Metchnikoff. It had been proved by this observer that certain mesodermic cells, which he called phagocytes, had the power of swallowing up other cells and bacteria, and that the bodies thus swallowed underwent a process of intracellular digestion. He showed that this power of the phagocyte cells was a most important one in connection with the question of immunity; for if a virus were inoculated into a resistant animal, the microbes were found to be rapidly devoured by the cells, whereas in the case of a non-resistant animal they remained free. Phagocytosis was thus confined to resistant animals. It would appear also that similar cells are charged with the defence of the human organism, and enter into conflict with the bacteria which infect it. It might be advanced against this statement that in some diseases, such as tuberculosis and leprosy, bacilli were found specially enclosed in cells, and yet the disease proved fatal. This might mean, however, that although in these diseases there is intense phagocytosis, the resistant power of the invader may under certain circumstances be too strong for the cells or the other means of protection. Even in fatal cases, however

signs of the struggle were not wanting,—certain of the cells were frequently found filled with dead bacilli. The establishment of the theory of phagocytosis did not necessitate an invariable victory on the part of the cells. The fight could, however, only be observed in immune or partially-protected animals, and was more evident in proportion to the degree of immunity.

The opponents of the theory might fairly ask two questions,—In what manner are the leucocytes attracted towards the microbes? Why are these cells, which in the case of immune animals destroyed the microbes, incapable of doing so in susceptible animals? In order to explain this, Metchnikoff, when he propounded his theory in 1883, made two assumptions,—1, that the cells were attracted to microbes and foreign particles in virtue of a special sensibility shown towards all foreign bodies introduced into the tissues; 2, that in the case of artificial immunity, the cells formed the habit during their preliminary struggle with an attenuated virus by virtue of which they were afterwards able to seize on the more virulent microbes. A simpler explanation was now, however, possible. Leucocytes might have the property, similar to that possessed by the zoosperms of the myxomycetes, of being attracted by some bodies and repelled by others. MM. Massart and Bordet had found that the products of microbes had a very marked chemical action on leucocytes. When a virus was introduced into the body it proliferated, and secreted a substance, which attracted the leucocytes to the seat of growth. In proportion as the power of the virus was greater, so much the more energetic became the poison secreted by it, till, in certain cases, the cells which flocked towards the bacilli became paralyzed by it and rendered incapable of taking up the microbes, which were, therefore, able to multiply without hinderance. Again, in certain diseases, such as chicken-cholera, the virus appeared to be still more powerful, and to exert a power on cells even at a distance, repelling them. This would explain the entire absence of phagocytosis in chicken-cholera. But if the inoculated animals had been previously rendered immune either by injections of attenuated virus or of certain doses of the bacterial products alone, then the conditions were changed. In this case introduction of virulent microbes was followed by an attraction of phagocytes towards the seat of inoculation, in the same way as occurred in other diseases, and these acclimatized cells possessed the power of enclosing and digesting the microbes before they had time to secrete a lethal dose of their poisons. The battle took place at the very commencement. If the leucocytes were not victorious then, their powers were afterwards entirely paralyzed by the defensive poison secreted by the microbes, which therefore gained the day. It would be seen, then, that every cause which interfered with the entrance of leucocytes to the seat of inoculation favored the chances of general infection. The theory of M. Metchnikoff did not, however, exclude other factors in the protection of the organism. It only claimed for the phagocytes a wider sphere of action, and a greater protective power than any other means. It seemed to explain all the observed facts, and far from being disproved by opposition, gained in probability with every fresh attack, which was a proof of its soundness.

After this admirable introduction, which was listened to with great interest, came very free criticisms from the partisans of the "humoral" theory. These seemed to be agreed as to the inefficacy of the cells, but are by no means at unity in regard to the actual factor in the blood or serum which produces immunity.

The first speaker was Dr. Buchner, of Munich, who said he could not accept Metchnikoff's doctrines, for four reasons:

1. Many observers had failed to observe phagocytosis even in white rats inoculated with anthrax (these animals being most resistant to the disease).

2. In diseases ending fatally, such as tuberculosis, mouse-septicæmia, etc., the microbes were frequently found in the cells, and yet death took place.

3. The experiments of Petruchky, Baumgarten, and others showed that anthrax bacilli, even if protected against the action of the leucocytes, perished in contact with the living fluids of the body of immune animals. He thought that the experiments of Metchnikoff, which seemed to show that these very fluids were an excellent pabulum for anthrax bacilli, could be explained by the fact that he introduced more bacilli than could be destroyed by the living fluids of his animals; a given quantity of serum had only a certain germicidal value. If certain microbes were placed in a given quantity of serum, one of two things might happen,—either they would be completely destroyed, or they would flourish luxuriantly, their fate depending on the number introduced. If, instead of placing them

directly in contact with the serum, the microbes were wrapped in sterile cotton-wool before introduction, and thus protected in some measure from the action of the great body of the serum, it would be found that after twenty-four hours they would begin to multiply vigorously. The bactericidal value of serum, therefore, disappeared soon after death.

Buchner, therefore, regards the migration of leucocytes to the point of inoculation as a secondary phenomenon. He supposes that the bacilli first wage war with the blood and serum, and that, being weakened thereby, they discharge the protein substances from their interior. These latter substances have been said to possess a powerful attractive force for leucocytes,—far more powerful than that of the products of bacterial growth in cultures. Thus all that Buchner will admit for the cells seems to be, that they may come to clear the field of the *débris* of weakened bacteria produced in their conflict with the serum. The facts which he advances to prove a connection between the immunity of a given animal against a given disease and the bactericidal action of its blood on the microbe producing the disease are the following: 1. The blood and serum of immune animals have a powerful bactericidal value. 2. The blood and serum of susceptible animals have no bactericidal power on anthrax bacilli. 3. The blood and serum of animals rendered artificially immune have a greater bactericidal power than those of unprotected animals. 4. A virus grown in the blood or serum of an artificially immune animal always becomes less virulent towards unprotected animals.

Dr. Behring was followed by Mr. E. H. Hankin, of Cambridge. This speaker claimed that he had foreseen the bactericidal power of serum from theoretical considerations, and did not regard it in any way as a vital phenomenon. He had isolated from the spleen and glands of immune animals a substance which he called cell globulin B, which had a most marked bactericidal value. He regarded the undoubted power of serum as due to the existence in it of ferment-like bodies capable of being isolated. He also gave an account of some experiments on the influence of diet on the susceptibility of wild rats to anthrax. Taking two series of four animals each, he fed one lot on bread paste, and the other on bread and meat, and after a time inoculated one of each lot with anthrax. Only the bread-fed animals died, and the others were then killed. He found in the spleen and glands of the meat-fed animals plenty of what he calls the defensive proteid, while in the other animals this was absent. He finds also that very young rats are not immune to anthrax, but can be made so by injection of the blood of their parents. The existence of these defensive proteids seems to him sufficient to account for the immunity in the animals in which they are found.

The crowning point of interest in the discussion was reached, however, when Professor Metchnikoff himself rose to reply to his opponents. He was greeted with most enthusiastic cheers, and had evidently been saving himself for this opportunity. Although generally—and partly, probably, because French is to him a foreign language—a very quiet speaker, he on this occasion spoke with extraordinary energy, and certainly with the language borrowed many of the oratorical manners of our French *confrères*. He considered that of all the arguments which had been raised against the theory of phagocytosis, the most important was that raised by Behring and Nissen,—viz., that the serum of guinea-pigs acclimatized to the vibrio Metchnikovii had bactericidal powers on the same vibrio. The serum of these animals would kill the vibriones in a few hours, although they multiplied freely in the serum of normal animals. However, was this fact, as Behring thought, capable of affording a complete explanation of the acquired immunity of protected guinea-pigs? Professor Metchnikoff declared that his own researches proved the contrary, for on studying the phenomena which occurred in the living animal, he at once noticed that the bacilli introduced into immune guinea-pigs remained alive for a very long time. In order to see this, it was only necessary to inoculate a pigeon with the vibrio Metchnikovii, and after a time to take a drop of the exudation from the seat of inoculation. This, when placed in a warm chamber and examined under the microscope, showed a large number of microbes enclosed in cells, and some also in the liquid portion. The cells died after their removal from the body, and one could observe the microbes developing in their interior, causing the cell to swell and eventually to burst, setting free the contained microbes. On taking a trace of this new and luxuriant culture and introducing it into the dead serum of an immune guinea-pig, not only was the serum unable to kill the vibrios, but it furnished a very excellent nutrient medium for them,—better even than that from an unprotected guinea-pig. Thus the study of the phenomena in living immune

animals furnished further evidence in favor of the phagocytosis theory. No theory of attenuation in the body of an immune animal, or of neutralization of the toxine, could apply to this case, for the microbes remained very virulent, and even immune animals are found to be quite as susceptible to the toxine of the vibrio as unprotected animals. One must, therefore, said the professor, never be content with studying the phenomena of immunity outside the organism. Buchner had insisted that in order to demonstrate surely the bactericidal power of serum it was necessary to take a small quantity of the microbe and mix it well with a large quantity of the serum in a tube. If the experiment were conducted as in the case in which cotton-wool was used to protect the microbes for a time from the serum, no bactericidal power was evident. Now, when we inoculated animals, did we not introduce a small mass of microbes under their skin, which did not spread freely in the blood and surrounding tissues, but remained localized somewhat as in the case of the cotton-wool protected microbes? In his opinion, this experiment of Buchner's threw no doubt on the phagocytosis theory, but was distinctly in its favor.

Summing up his researches, he came to the conclusion that phagocytosis occurred whenever an animal recovered from an infectious disease; the death of the animal was proof of the absence or insufficience of the process of phagocytosis.

I would have wished to continue this letter for several columns, but I know that the demands on space are many. I hope, however, to devote part of my next letter to a continuation of the study of some of the points raised in this highly successful congress.

Notes and Queries.

INFLUENCE OF ALKALIES ON THE BILE.

W. N. NISSEN has recently undertaken some experiments as to the influence of the alkalies on the secretion of the bile on a dog with biliary fistula. His results are recorded in *Les Nouveaux Remèdes* for August 8, 1891. After having first satisfied himself that the quantity of bile secreted was subject to but little variation, and that water, even when administered in very large amounts, was without influence, he proceeded to study the action of sodium chloride, sodium sulphate, bicarbonate of sodium and the acetate of potassium, phosphate of sodium, sulphate of magnesium, and Carlsbad salts. From his experiments it results that of the alkalies, concentrated solutions considerably diminish the quantity of bile excreted. In dilute solution—one-half to one per cent.—they produce no influence upon the secretion of bile; the acetate of potassium is most active in reducing the biliary secretion, which may be fifty-seven per cent. below normal under the action of this drug. Its action, however, is only transient. The author believes that the diminution of the biliary secretion under the influence of the alkalies is due to impoverishment of water, for he finds that the more concentrated the alkalies the greater the diminution in the amount of the bile, which becomes more concentrated and the proportion of coloring-matter in the bile becomes increased. The therapeutic action of alkalies in jaundice is not attributable to their cholagogue action, but to the diminution in the formation of the bile and to their solvent action on the mucous membrane, which constricts the biliary ducts. In jaundice the author advises the association of potassium acetate with Carlsbad water.

CAUSE OF THE FIRST SOUND OF THE HEART.

DR. BERRY HAYCRAFT (*Journal of Physiology*, vol. xi. No. 6) maintains that the first sound of the heart is valvular. He finds that the sounds of the excised and bloodless heart—in which, of course, the valves do not come into play—are much lower in pitch than the first sound. In an experiment on the excised heart of a calf he rendered at the same time both the auriculo-ventricular and the aortic valves equally and suddenly tense by means of a stream of water, and found on auscultation that the sound of the auriculo-ventricular valve was a minor third lower than the sound of the aortic valves. He does not deny that the valvular first sound is accompanied by the so-called muscle sound; but a muscle is not like a piano-string, vibrating so as to produce a musical note; its motions are merely transmitted to the middle ear, causing vibrations of the membrana tympani, and it is these that are heard. The muscle plays the part of a drumstick, not of a drum; and when one listens to an excised beating heart and hears a low impure musical note, it must not be imagined that this is given out by the muscle, for it is the resonance note of the stethoscope and ear that is heard, called forth by the moving mass on which it is placed. When

the heart is within the chest, both the resonance tones of the chest, the stethoscope, and the ear are heard; whereas in auscultation, wherever the stethoscope is placed and whatever instrument is used, the valvular high-pitched note of the first sound is always the same; the accompanying resonance notes, on the other hand, may vary. These are rounded and lower in pitch over the epigastrium, higher and shallower over the hard ribs; they vary with the stethoscope used, and may be raised in pitch by inflating the middle ear with air through the Eustachian tube.—*British Medical Journal*, August 22, 1891.

DEPILATORY POWDERS.

DR. CLASEN says (*Monatshefte f. Prakt. Dermat.*, 1889, ix. 541) that among the best depilatory powders are sulphohydrate of sodium and sulphide of barium. As to the sulphohydrate of sodium, he says that, used as a paste, one part to eight of water, and allowed to remain on for a very short time, it acts well. But it deteriorates very rapidly and is dangerous to give to a patient, as it is quite capable of producing scars. The sulphide of barium is a safer powder for the purpose. It may be used by mixing fifty parts of it with twenty-five parts each of starch and oxide of zinc. This is mixed with water so as to form a soft paste and spread upon the face. After ten minutes it is scraped off, and leaves a smooth skin.—*Medical Tribune*, August 15, 1891.

BORAX AND CHLORAL.

The growing practice of ordering "borated chloral," or a mixture of borax and chloral, for certain throat affections, has induced a French pharmacist, M. DUJARDIN, to publish a note calling attention to the fact that the product supplied will vary with the temperature at which it is prepared (*Bull. Gén. Thèr.*, July, p. 91). When the solution is made in the cold, as the borax is without perceptible action on chloral at a low temperature, the latter will remain intact in the solution. With the application of heat, however, the two compounds will react and chloroform will be liberated from the chloral and may remain in the liquid. Should the liquid be allowed to boil for a few moments, the whole of the chloral would be decomposed, and the resulting chloroform would volatilize. M. Dujardin, therefore, recommends that if in making this preparation heat be used to dissolve the borax, the solution should be allowed to cool before adding the chloral.—*Pharmaceutical Journal and Transactions*, August 29, 1891.

CREOSOTE GIVEN BY THE RECTUM.

DR. REVILLET (*Gazzetta degli Ospitali*, 1891) administers creosote by means of rectal injections. He uses the following:

℞ Creosot. purissim., gm. 2–4 (gtt. xxx = ʒi);
Ol. amygdalar. dulc., gm. 25 (ʒvi);
Aq. destillat., gm. 200 (ʒvii);
Vitell. ovi., gm. 1.

The injections are generally given in the evening before the patient retires, after a preceding injection to clean out the intestine. Absorption takes place at once, the patient notices the characteristic taste, and the urine assumes the blackish or black-green color. At the same time the temperature falls, as after antipyrin. The injections are well supported and absorbed. It is taken up by the branches of the portal vein and eliminated by the salivary glands, pulmonary mucous membrane, and, above all, by the kidneys. One should be certain of the soundness of the liver, heart, and kidneys before prescribing creosote in large doses. In tubercular diarrhœa the injections seem to arrest the discharges.—*Cincinnati Lancet-Clinic*, September 19, 1891.

FOR RHEUMATISM AND LUMBAGO.

HOLLISTER gives the following in doses of 1 teaspoonful three or four times a day:

℞ Potassium iodide, 15 gm.;
Potassium bromide, 15 gm.;
Tincture of colchicum seed, 30 gm.;
Syrup of bitter orange-peel, 50 gm.;
Distilled water, 150 gm. M.

Sig.—The dose should be augmented until the bowels are moved.

—*National Druggist*, September 15, 1891.

SALICYLBROMANILIDE A FRAUD.

Antinervine was the name under which RADLAUER, a Berlin apothecary, launched, some years ago, what pretended to be a new analgesic and antipyrĕtic, the chemical designation for which, in conformity with its alleged constitution, was given as salicylbromanilide. The inference was that this body was analogous to bromacetanilide, the salicylic acid residue having taken the place of the acetyl residue. Having become suspicious of this substance, E. Ritsert subjected the same to a rigid examination

(*Pharm. Zeit.*), and found, not a chemical unit, but a mechanical mixture of salicylic acid, ammonium bromide, and acetanilide. A number of samples, some older and some new ones, were analyzed and gave approximately the following composition:

Salicylic acid, 25;
Ammonium bromide, 25;
Acetanilide, 50.

—*Western Druggist*, August 15, 1891.

A NEW ANÆSTHETIC.

It is a long time ago since John Hunter, when cutting into the cock's comb, noticed the anæsthetic effects of cold. Although his primary object was to restrain bleeding, he nevertheless remarks that the operation was apparently painless. Dr. Richardson also, noticing the benumbing effects of cold, devised his apparatus for its artificial production by spraying ether on the part to be operated on, but from some cause ether spray fell into disfavor with surgeons, and was finally superseded by cocaine as a local anæsthetic. Cocaine, however, has not fulfilled all the promises held out for it by its advocates, it is not perfectly innocuous, and it has been blamed for delaying the healing of the cut surfaces of the part anæsthetized by it. The search for a new anæsthetic, local in its action and non-poisonous, was recommended, and, if we accept M. Monnet's statements, we have in the chloride of ethyl all that is sought. Already it has found an advocate in Dr. C. Rédard, Clinical Professor at the Geneva School of Dentistry, who speaks highly in its praise.

Chloride of ethyl, C_2H_5Cl, is a colorless mobile liquid, having a peculiar and pleasant odor, and a sweetish, burning taste. It boils at 12.5°, at 0° possesses a specific gravity of .9214. It is slightly soluble in water, but dissolves readily in alcohol. It is sent out for medicinal use in hermetically sealed glass tubes containing a little more than two drachms each. When required for use, the point of the tube is snipped off, and the warmth of the operator's hand is sufficient to cause a very fine jet of the chloride to be projected on the part to be anæsthetized. No apparatus is required,—a great advantage. Up to the present its use has been confined to dentistry and as an external application in neuralgic affections, but there is little doubt that in a short time its value will be tested in general surgery. Although the use of the chemical as an anæsthetic is new, the chloride is one of our oldest preparations. Of course, it was not known as chloride of ethyl, for the name "ethyl" was the invention of Berzelius, and the word "chloride," a little later (1812), by Sir H. Davy. But in the beginning of the fifteenth century, at the end of the ecclesiastical era in medicine, and the dawn of the Renaissance, when Brunelleschi commenced the task of reviving the love of the beautiful by copying Greek architecture and Greek statuary, even prior to the birth of Linacre, Basil Valentine, by distilling common salt and spirits of wine, having, as he says, previously united them, obtained an alcoholic solution of the chloride. "Thus is the *spiritus salis et vini* prepared." And Johann Rodolph Glauber, better known as the maker of the "sal mirabile" (Glauber's salt), in 1648 wrote, "When dephlegmated spirit of wine is poured in strong spirit of salt and digested for a long time, the spirit of wine makes a separation and kills its *sal volatile* so that a fine clear *oleum vini* swims on the top, which is not the least potent of the cordials."—*Medical Press and Circular*, August 12, 1891.

FOR WARTS ON THE SKIN.

H. ROEMER, of Henfeld, Austria, writes to the *Pharmaceutische Post* that he has frequently used, and always with success, the fresh flowers of Verbascum phlomoides and of Verbascum thapsus (mullein) against warts. The flowers, freshly torn from the calix, are pressed upon the wart with the fingers, and finally rubbed smartly. The procedure should be repeated if one or two rubbings do not suffice. In the great majority of cases, one rubbing suffices.—*National Druggist*, September 15, 1891.

DIGITALIN AND IODIDE OF POTASSIUM.

These two remedies have recently been attracting considerable attention from the point of view of their cardiac action, and many points have been elicited through the study of French observers, which tend greatly to elucidate their action and to show more clearly the cases in which they may be employed to advantage. In *La Médecine Moderne* for July 30, 1891, DR. GERMAIN SÉE takes up the study of these drugs, and shows that digitalin is not a remedy which increases the energy of the heart, but acts by calling into play the elasticity of the cardiac muscle and exaggerating

the diastole of the left ventricle, which thus receives a larger amount of blood from the ventricles, and consequently expels a larger amount of blood into the arteries. As a consequence blood-pressure is elevated and the general circulation facilitated. Digitalin thus constitutes a typical remedy for cardiac lesions where there is disturbed compensation, its effects being especially manifested upon the right heart, and thus tending to remove disturbances of the lesser circulation. Iodide of potassium, which one would think was far removed in its effects from digitalin, nevertheless seems to act in the same manner by at first increasing blood-pressure as a result of the potassium element, and then enlarging the arterial area through vascular dilatation from the action of the iodine. Hence the great value of potassium iodide in dyspnœa, whether of asthmatic or cardiac origin. Dr. Sée thus believes that digitalin produces dilatation of the heart, although we must admit that clinical evidence largely tends to contradict this view. So, again, as regards the action of digitalin upon the two hearts, we can find difficulty in accepting the deductions of Sée.

ROSSBACH'S EXPECTORANT MIXTURE.

Apomorphinæ hydrochloratis, gr. i;
Morphinæ hydrochloratis, gr. ss;
Acidi hydrochlorici dil., gtt. x;
Aquæ destillatæ, f℥v.
Dose.—A teaspoonful every two, three, or four hours.

The diluted hydrochloric acid is evidently superfluous.—*American Druggist*, August 15, 1891.

TREATMENT OF AFFECTIONS OF THE EAR WITH BETA-NAPHTHOL.

Dr. Hand recommends this remedy in the treatment of the majority of chronic suppurative affections of the middle ear, maintaining that the suppuration will be arrested after six to eighteen days of its employment. He does not advise its use in acute affections, but its beneficial action is especially manifested in subacute and chronic affections and in non-complicated otorrhœa. It is, above all, indicated in tubercular lesions of the mucous membrane of the middle ear. His method of employment is as follows: The tympanic cavity should first be washed out with a warm antiseptic solution, the middle ear then insufflated, and the auditory canal and the tympanic regions then carefully dried with absorbent cotton. A small quantity of the naphthol is then blown into the middle ear, and the external auditory canal closed with a pledget of aseptic cotton. When the secretion is very dense, or when the openings in the tympanic membrane are situated high up, or when there are exuberant granulations as well as furunculosis, Dr. Hand employs an alcoholic solution of naphthol, one and a half to three per cent., which is injected into the ear twice daily, and allowed to remain for a few minutes.—*Journal de Médecine de Paris*, September 6, 1891.

BANANAS IN CHRONIC BRONCHITIS.

In cases of chronic bronchitis with difficult breathing and scanty expectoration the use of banana-juice has been highly praised. The juice is prepared by cutting up the bananas in small pieces, and putting them, with plenty of sugar, in a closed glass jar. The latter is then placed in cold water, which is gradually made to boil. When the boiling point is reached the process is complete. Of the syrup so made a teaspoonful every hour is the proper dose.—*Medical and Surgical Reporter*, September 5, 1891.

THE PHYSIOLOGICAL ACTION OF RUBIDIUM-AMMONIUM BROMIDE.

Drs. Tausk and Vas report, in the *Deutsche Medizinal-Zeitung* for August 31, 1891, some experiments which they have made with rubidium-ammonium bromide. The smallest dose which they found would produce any appreciable result was ⅓ grain. They found that the subcutaneous injection of 5 grains in a frog produced fibrillary contractions at the point of application, which soon extended to the other muscles of the body and produced convulsions, which, however, lasted but a few moments. And after four or five minutes the movements of the animal became sluggish and respiration weaker. The hind extremities became anæsthetic, and this loss of sensibility then extended to the anterior parts, so that after twenty minutes there was anæsthesia of the entire body, with the hind extremities paralytic, death resulting in about forty minutes. In larger doses these symptoms appeared more rapidly and with greater intensity.

In mammals the symptoms which followed an injection of this double salt are about the same. After the injection of fifteen grains in the rabbit, clonic convulsions occur, which, after five or ten minutes, give place to general relaxation and anæsthesia, at first of the

anterior and then of the hind extremities. The pupils are dilated, the pulsations of the heart more rapid, and the respiration slowed. After prolonged administration of this remedy, rabbits acquire a toleration of this drug, even to larger doses, yet symptoms of marked motor ataxia are present. The authors divide the action of the bromide of rubidium and ammonium into a local and a general action. The first, less pronounced than the second, consists in a fibrillary contraction at the point of injection, or when administered internally in catarrh of the gastric mucous membrane. Among the general actions, the most marked is its action on the respiratory centre. In rabbits it produces the characteristic periodical respiration described by Biot. It also produces anæsthesia of the cornea and of the mucous membrane of the mouth, with marked modifications of the spinal reflexes, which are first increased in intensity and then decreased, and then finally abolished. The activity of the heart is at first increased and then decreased. After large doses, temperature and body-weight steadily decrease.

THE ACTION OF STRYCHNINE ON THE CEREBRUM.

The majority of authorities attribute the action of strychnine to its influence on the spinal cord and medulla oblongata. These conclusions are warranted not only by the character of the symptoms produced in strychnine-poisoning, but also by the fact that after fatal poisoning by strychnine large amounts of the alkaloid are to be found in these localities, while but a trace is detectable in the cerebrum. In addition, there are no direct experiments as to the action of strychnine on the cerebrum, and but few observations have been attempted in this connection, and they are mostly of a negative character.

In the *Centralblatt für Klinische Medicin*, No. 36, 1891, is an account of some experiments by Dr. Biernacki, who has tested the action of strychnine on the cerebrum by exposing the motor centres of the cortex, determining the weakest possible stimulus which would produce a motor reaction, then injecting strychnine, and testing the changes in irritability. His results show that strychnine produces marked reduction in the irritability of the cortex, whether administered subcutaneously or locally applied. It is not clear, however, as to whether strychnine has a direct influence on the gray substance of the cortex or not. Since it is possible that the apparent reduction in irritability may be due to the action of this alkaloid on other parts of the central nervous system.

A NEW SOLVENT OF CAMPHOR.

From the frequency with which the indications for the subcutaneous injections are met with, it is evident that a good and reliable solvent for this substance is a great desideratum. Ethereal solutions rapidly evaporate. Alcoholic solutions also evaporate, and the camphor becomes precipitated, so that injections of such solutions produce severe pain or even abscess. Solutions of camphor in oil are difficult to employ, while besides possessing the disadvantage of the liability of becoming rancid.

In the *Zeitschrift für Therapie* for September 1, 1891, Dr. Karl Rosner recommends in the highest terms a solution of camphor in liquid paraphine, which, when slightly warmed, forms a perfectly clear and limpid solution. He states that he has kept this solution for more than five years without its properties becoming changed.

TREATMENT OF URTICARIA.

Dr. Quinquaud treats urticaria by the internal administration of alkalies, and, if this does not prove successful, by the administration of arseniate of sodium or naphthol. When the itching is very depressing, he uses a wash composed of 30 parts of boric acid and 5 parts of chloral hydrate in 180 parts of distilled water; or he uses a powder containing 5 parts of salicylic acid, 15 parts of oxide of zinc, and 30 parts of powdered starch. — *Internationale Klinische Rundschau*, August 16, 1891.

THE LOCAL APPLICATION OF CARBOLIC ACID SOLUTION IN THE TREATMENT OF ARTICULAR RHEUMATISM.

In the *North American Practitioner*, for September, 1891, Dr. Joseph Lane Hancock writes that for the last two years he has been treating cases of inflammatory rheumatism with a local application of carbolic acid applied in the form of a four per cent. solution on a warm flannel cloth wrapped closely around the entire affected joint.

Dr. Hancock states that his custom is to leave this dressing on over night, placing it in position just before the patient retires.